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EMPIRICISM AND RATIONALISM[1]

MEMBERS of the Harvey Society, ladies and gentlemen, I have been asked to discuss the proper manner of treating data from a statistical and mathematical viewpoint and I have chosen as the precise wording of my topic the more general formulation "Empiricism and Rationalism," to the end that I might emphasize a distinction in point of view between methods, and more generally between aims, in the treatment of data by statistical or mathematical analysis. For I believe that without a keen appreciation of the distinction between empiricism and rationalism it is impossible properly to understand the problem of the treatment of observational material.

When we seek for definitions of empiricism or rationalism we may well turn to the Century dictionary in which the philosophical definitions were formulated by Charles S. Peirce, an expert in making refined physical observations and in reducing them, and a great logician and philosopher. I understand that in the medical sense empiricism is quackery, so at any rate the Century dictionary states, but this part of the definition may not be due to Peirce. We find the following:

> Empiricism—3. The metaphysical theory that all ideas are derived from sensuous experience—that is, that there are no innate or *a priori* conceptions.

And again:

> Rationalism—3. In metaphysics the doctrine of *a priori* cognitions, the doctrine that knowledge is not all produced by the action of outward things upon the senses but partly arises from the natural adaptation of the mind to think things that are true.

You will notice the difference between these definitions. It isn't that empiricism emphasizes the importance of sensuous experience. It is that it states that all ideas are so derived and that there are no innate or *a priori* conceptions. This notion is not unfamiliar; one finds it expressed by a good many writers, and particularly by writers in the biologic fields. Some seem to hesitate a little bit at the extreme form of the statement and to qualify it by some sort of assumption that there may be an inheritance of ideas, so that empiricism should be stretched to

[1] Lecture delivered before the Harvey Society on February 6, 1926.

include not only the sensuous experience of the individual but the sensuous experience of the race as transmitted to the individual. It seems to me that if one so stretches the notion one might almost as well give it up; because it is hard to see wherein sensuous experience derived through evolution of the race should differ from innate or *a priori* conceptions. In fact, one might almost maintain that innate and *a priori* conceptions are precisely the quintessence of the sensuous experience of the race. We shall therefore cleave to the original extreme form of the statement that all ideas are derived from sensuous experience and that there are no innate or *a priori* conceptions.

Rationalism, on the other hand, does not say that all knowledge arises from the natural adaptation of the mind to think things that are true. It states that there are *a priori* cognitions, that knowledge is not all produced by the action of outward things upon the senses but partly arises from the natural adaptation of the mind to think things that are true. It is therefore not precisely the antithesis of empiricism. That antithesis would be found more nearly in the extremist interpretations of the idealism of Berkeley where the existence of external things is made to depend on their perception by the mind. Rationalism is a sort of middle ground and as such might readily be assumed to be nearer the truth than either extreme, empiricism or idealism.

We are very prone to extremes and I would not deny that very much advance in science and in philosophy and in art has been made by the struggles of the extremist of one sort or another to prove that a single point of view is adequate for the systematic formulation of a philosophy. As a matter of fact the extremists on both sides are apt somewhat to ridicule the moderate position of any one who occupies intermediate ground; he is, so to speak, between two fires. He has perhaps not the same initiative of attack, not the same uncontrolled zeal of the extremist and this constitutes for him a certain weakness or vulnerability. We are prone to follow special pleaders, whether in religion or in science or in ethics. I might liken empiricism to one end of the spectrum, let us say, the infra-red, and liken idealism to the other end, the ultra violet, and then I should characterize rationalism as constituting the visible light. And I have an idea that we can not see nature whole in any monochromatic light, whether visible or invisible. Our own interests may be important, but so are the other interests of other persons.

One aim of statistical and mathematical analysis in the treatment of observations is the empirical aim of describing our experience. If we have a large number of observations we may wish to describe them by certain characteristics of the whole group. This leads to using the mean or median to express the center of the group or rather some center of the group. We use other constants, for example, the standard deviation or the probable error or the interquartile range to express a measure of the scattering of the individuals of the group from their center. We can determine other characteristic constants of the group. This is purely descriptive statistics. Its value lies in enabling us to replace the great variety of the group of observations by a lesser variety of somewhat technical descriptive constants computed from the elements of the group.

In other types of problems we need the empirical equation. We have one variable which depends more or less upon another and we make a plot to show the values of one variable coordinated with those of the other. If the values run fairly smoothly we draw a curve threading among them in such a way as to satisfy our esthetic judgment as to the probable relation between the variables. For many purposes such a graphical delineation of the smoothing process may be adequate. But even when it is adequate and in many cases when it is not we have recourse to the empirical equation—which means that we select some type of mathematical expression which in a general way runs along the graphical curve and which contains a certain number of parameters that may be assigned, by one method or another, such values as to make the analytical expression lie extremely close to the observations.

In case there is a great deal of scattering among the observed relationships such as we should find, for example, if we undertook to plot the heights and weights of different individuals, we may have recourse to decidedly complicated methods of calculating what we consider to be the best curve to represent the relation between these variables when abstraction is made from the accidental variations of each variable. This field of effort may be generally subsumed under the title of correlation. We should not restrict this definition to imply that the regression equations need be linear.

In all these cases, whether we are content with representing the characteristics of a group by a few statistical constants, whether we describe the cogrediency of a pair of variables by a graphical or analytical smooth curve, or whether on account of the greater scattering we combine these two notions into the general notion of correlation we are still in the domain of description or of empiricism. We are in the domain which is represented, for example, in botany by the herbarium with the dried plants attached to the sheets with their appropriate descriptions and filed away for reference. We are in a museum.

There is, on the other hand, the rationalistic point of view in almost all science, namely, the effort to apply original thought to the explanation of the relationship between variables. In a certain sense an explanation means a search for causes, and in a certain sense one may maintain that there are no causes; that throughout nature there is only concomitancy; that those who speak in terms of forces and causes are merely using a different kind of description or a different extent of description from the frank empiricists; but certainly the aim of the person who undertakes to discover natural laws, so-called, is somewhat different from the aim of him who undertakes to describe. Their methods also differ. Ordinarily the empiricist multiplies description until it becomes more and more realistic. Ordinarily progress in the rationalistic direction is made by ignoring the lesser variations which may be assumed to be due to accident, or at any rate to lesser causes, and by focusing the attention upon an ideal situation where only a few major causes are working; that is, rationalism proceeds by idealization, whereas empiricism proceeds by realization. For the rationalist it may be a positive handicap to know too much in detail the relations which exist in nature. Often the great generalizations come early. Isaac Newton perhaps had a simpler problem before him when he had the observations of planets as reduced by Kepler and systematized into the three laws of Kepler than he would have had if he had been in possession of knowledge of all the multifarious perturbations introduced in the orbits of each planet by the influences of all the others. You can think of many such cases in the biologic field.

This crucial notion of the rôle of idealization in the discovery of natural law may be exemplified by any number of instances. Consider, for example, the question of motion and of force. The fact of observation is that all moving bodies come to rest unless some effort is expended in maintaining the motion. Prior to the time of Newton this universal experience was interpreted as meaning that a forward force was acting on all uniformly moving bodies. Newton said, No, that which stops the body is in the nature of a resistance, bodies left quite alone must persist in uniform motion. Such an idealization requires insight. It may be doubted whether Newton got it from his sensuous experience. It is possible that he contributed this idea, and that we are here in the presence of a mind especially adapted to penetrate behind the deceptions of things as they seem and to think things as they are. Lavoisier's law of the indestructibility of matter or conservation of mass is another case of reversing the obvious to find an idealization. Fortunately for the advance of science the reversal of an accepted point of view is not necessary to the discovery of a law of nature, but a persistent intensity of original thought directed toward the formulation of an ideal situation undisturbed by accessory happenings does seem essential. Moreover, one must have the intuition to decide rightly what is accessory and what is fundamental in the problem considered. And further, he must have a feeling for what are the present problems that are worth while.

So long as persons merely observe nature, howsoever intently, and describe, howsoever accurately, that which they observe they experience real difficulty in discovering natural laws and in confirming their discoveries. This is due to nature's infinite variety. It is the experimental method which has so advanced science by leaps and bounds. The experimenter can somewhat control conditions, he can limit the accessory variations, he can repeat and vary his experiments until a general inference becomes possible.

I believe that Maxwell, contemplating the great complexity of the spectrum, once remarked that given a mathematician of sufficient ability a wonderful contribution to our understanding of the constitution of matter could be made by the mathematical analysis of the spectrum. Scientific history now tells us that better experiments, sharper eliminations of the complexities, closer attention to the simplest cases, proper and new coordinations of idealized physical concepts and relatively simple mathematics have set us on what we believe to be the right track. This is, I venture to think, the usual way of advance—idealization, a recombination, sometimes a reversal, of scientific concepts, new experiments, and a little mathematics. It is the breeders, Mendel with peas or Morgan with Drosophila, who urge genetics forward, not the sociologist or statistician. The place for complicated mathematics is in the follow-up, in the codification of the whole field.

What is mathematics? Every mathematician knows, but few others realize that mathematics is but the details of the tree of logic. Indeed, one may say that mathematics form the branches, the twigs, the leaves of the tree of deduction of which the trunk is our everyday logic and the roots are those dark intricacies of the professional logician. Mathematics is not science; it is not nature, unless it be in the nature of the mind; it is not concerned with the truth but only with the exactness of the deductive process. The confusion of many a scientist with respect to what mathematicians can do for his subject is due to the historic fact that in the past mathematicians have been for the most part interested in the application of their methods to natural phenomena, they have been astronomers, physicists, physiologists working with analysis as a tool. Mathe-

matics is an affair not of empiricism nor of rationalism but of idealism. Once a scientific problem has been formulated in exact quantitative premises which may be converted into formulas, mathematics may go at its deductive processes and may or may not arrive at valuable conclusions from those premises. Its chief use is in the follow-up, in the codification, in the elaboration of scientific advance, not in the original discovery. And as far as its limited use in discovery goes, it must be mathematics in the head rather of the scientific discoverer than of the mathematician.

If you rightly seize my point you will know the importance of having any young man who may contemplate a life devoted to rationalistic science acquire in his college course a knowledge of mathematics through the elements of the differential and integral calculus. If he is fortunate he will need that knowledge sooner or later. He may meanwhile have forgotten most of the detail he once learned, but under the stress of necessity and the stimulus of his problem he can recall as much of the general principles as he is likely to need. Happily that type of mind from which spring the rationalistic advances of science is usually so constituted that it can acquire even in maturity those simple mathematical notions which may have become indispensable; it can almost invent them for the concrete instance at hand. Ronald Ross and Galton are examples; but the process of invention is harder than that of recollection. And again, if you have seized my point you will understand why in acceding to your request that I discuss the statistical and mathematical methods of treating data, I have come to you with general ideas, with points of view, with distinctions in aims, instead of with formulas. All formulas are technical details of problems already formulated, and it is the formulation which is at once the more difficult and the more important part. Runaway mathematics is like a runaway horse in doing nothing but harm to itself and others; it is irrational.

To return to our subject. We have mentioned empirical equations and laws of nature which may be converted into formulas. What is the distinction between them? If we were interested in smart dialectic we might launch forth on a demonstration that there is no such distinction, that from the most empiric of equations one may proceed by imperceptible gradations to the most universal of natural laws as from darkness to light. But this tour de force would not be a useful contribution to our present discussion. Better is it to contrast the more empiric with the more rationalistic. An empirical equation is a mathematical expression containing in addition to the variables certain parameters or adjustable constants which may be so chosen as best to represent the data. For example, suppose you plot the complete expectation of life at birth at different times, say every decade, from 1850 to 1920. You will naturally take the time as the abscissa or horizontal variable and the expectation of life as the ordinate or vertical variable. You all know what you will see, namely, a series of points rising fairly steadily from a value between 40 and 45 to a value between 55 and 60, representing an increase of about fifteen years in expectation in seventy years. You may smooth this series of points graphically. You may fit some equation to them, for example, a linear equation, as "expectation = a + bt," where the time t is measured in decades from 1850, and where a and b are the two parameters to be chosen to fit the data, as expectation = 43 + 2t. Such an equation is empirical. It describes roughly the variation of expectation that has been experienced during the time in question. If the graph appears to show a general curvature with an upward acceleration and if you are a little more adept in curve-fitting you may venture to try a parabolic or quadratic form of relationship such as "expectation = a + bt + ct^2" and on determining the constants a, b, c by any method, say by a simple trial until you have as satisfactory a shape for the curve as seems possible, you will again have an empirical equation to represent the facts.

Nobody, however, should now assume that he has discovered a veritable law of nature. It is not a law of any generality that the increase of expectation is linear, is proceeding universally and always has proceeded at a uniform rate, or with a uniformly accelerated rate. It may be interesting, but it is not scientifically valid, to produce or extrapolate the empirical

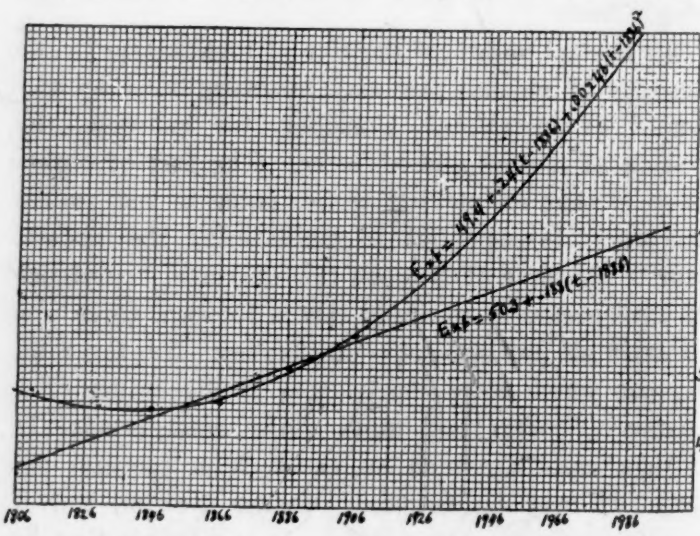

Fig. I

equation forward to 2000 A. D. or backward to 1800 to ascertain what are the expectations of life for babies born as of those dates. No analysis has been made of causes, no detailed analysis has been made of the ways in which the change of expectation has come about. There has been merely a gross description of a total phenomenon; it is

graphic, but not the basis for an induction. Figure I represents such a treatment of some Swedish data which are at hand.

EXPECTATION OF LIFE AT BIRTH IN SWEDEN AT SPECIFIED DATES

Fitted by I. $\text{Exp} = a + bt = 50.3 + .188\ (t - 1886)$
and by II. $\text{Exp} = a + bt + ct^2 = 49.4 + .24\ (t - 1886) + .00246\ (t - 1886)^2$

Year	Expectation	Calc. by I	Diff.	Calc. by II	Diff.
1846	44 years	42.8	− 1.2	43.8	− 0.2
1866	45 "	46.5	+ 1.5	45.6	+ 0.6
1886	50 "	50.3	+ 0.3	49.4	− 0.6
1896	52 "	52.2	+ 0.2	52.0	0.0
1906	55 "	54.1	− 0.9	55.2	+ 0.2

You see that the linear fit depending on two parameters is not bad, that the quadratic description depending on three parameters is excellent and that either, when produced backward or forward, gives within relatively short lapses of time results which themselves challenge belief in the generality of the description.

There are in the literature many instances of this sort of treatment of data, and I am tempted at times to fear that they may do more harm than good—not that in themselves they are bad, but because they allure readers toward illegitimate generalizations and forecasts. There is a physician of my acquaintance who makes it a rule when reporting his cases in print never to express his results in percentages. His reason is that figures in percentage imply a generality in his experience which he neither posits nor feels. The emotional reaction to a mathematical, even to a simply arithmetic statement, is often extreme. Many persons like to tease others into the spinning of yarns; under such provocation figures and formulas make first-rate liars; but why one then takes them so seriously I can not imagine.

As another illustration of an empirical equation we may take from the field of epidemiology Farr's Law, with which you are all familiar. The law states that, other things being equal, the death-rate increases with the density of population, and that in particular it increases as a root, say the tenth root, of that density. Other things never are equal, and the law is of relatively little use in estimating the death-rate from the density of population. Moreover, it is obviously a law of restricted application because it gives impossible results when extended to practicable differences of density. For example, there are rural districts in which the number of persons per acre is certainly not one thousandth of that in this great metropolis. The tenth root of 1000 is 2. Yet we should not expect to find a death-rate here, even when carefully adjusted to the age distribution of the population, twice as high as in those rural districts. Nevertheless, with reasonable allowances, there is a considerable degree of truthful generality covered by Farr's Law. Insofar as the law is sound it should have, even though empirically derived, some rational basis. Rationalistic science advances not merely, perhaps not mainly, by the discovery of laws through reasoning, but rather more by the invention of reasons for those uniformities or generalities which may have been observed. One may attempt to rationalize Farr's Law. He may point out that persons can not die of diseases they do not have, that many of our illnesses are infections and that the chance of contracting such diseases is greater in a denser population, so that the death-rate, other things being equal, might have been expected to be higher with the increase of contacts and the faster and further spread of infections. One might show that pneumonia is an urban more than a rural disease.

Such arguments and illustrations abound in the literature and indeed in our everyday thinking; they are evidences of a well-nigh universal tendency, an admirable tendency, of the mind to rationalize our experiences; they are enticing but must be critically examined. To avoid the pitfalls of plausibility, persistent thinking and varied experience must be applied. Rationalism is not thought *per se*, it is thought applied to observations. In an examination of the situation with reference to cancer in Massachusetts conducted during the past summer, it was found that in the main the mortality from cancer obeyed Farr's Law. It nearly doubled from the districts of lowest to those of reasonably high density of population, but did not seem further to increase at the highest densities. This may not play into the hand of a rationalization of the law upon the basis of contacts and infection; it may point to general living conditions with reference to light and air and outdoor work as contrasted in rural and urban living and dying. Premature or immature rationalization of empirical formulas is to be rated with the extrapolation of such formulas as an error of empiricism, and as one which approaches to quackery.

The statement of a law of nature as a formula ordinarily contains certain constants or parameters. Thus the gas law $pv = Rt$ uses the gas constant R. Some constant is necessary because of the arbitrariness of the units in which pressure, volume and absolute temperature may be measured. We know that with the units customary in physical chemistry the gas constant R is about two calories; its value when any other system of measures is used can be obtained by simple arithmetic. Such a constant is called a constant of nature or a universal constant. Its value has of course

to be obtained from experiments and in this sense it is a fitted constant; but once determined it is fixed and must be assumed whenever the gas equation is used; it is not an adjustable parameter of the sort found in purely empirical equations. Again, the law of gravitation states that the attraction varies as the product of the masses and inversely as the square of the distance. Converted into a formula, $F = \gamma m_1 m_2 / r^2$. Here the multiplier γ is a universal constant. The values of the masses may be determined by experiment or, in celestial mechanics, by observation, and in this sense those values are fitted constants, but they are not general empirical parameters; you may not use one value of the mass of the earth in the solution of our motion about the sun and then use a different value in treating the motion of the moon about the earth simply because a different value may give a better fit. If Farr's Law that the death-rate is equal to a multiple k times the nth root of the density of population were a full-fledged law of nature both k and n would be universal constants, of which the first would depend in numerical value on the units employed in expressing death-rates and densities, and n, the root, would be a specified number. The search for laws of nature is in part a search for universal constants.

It should go without saying that most of our natural laws are but fledglings. They or their followers will mature with further investigation and in due time. As they stand to-day, particularly in fields of science just coming under quantitative treatment, they will contain fittable parameters. Indeed the further advance of science may demonstrate that some of the constants must remain fittable. This may be illustrated by recalling Kepler's Law that the planets revolve about the sun in ellipses with the sun at one focus. He left the eccentricity, the orientation and the size of the ellipse undetermined. These remain adjustable constants to this day. The law of gravitation does not supply them. They are constants of integration, constants which arise in passing from the differential equations of celestial mechanics, equations involving accelerations, to the integrals which express first velocities and then positions. Although as early as Galileo and Borelli some attempt at deductive physiology was made, and many efforts have been since directed and are even now most intensively being prosecuted toward such an end, it is too much to hope that we shall soon reach a physiology as rationalistic as mechanics. The living organism is not so simple as a chunk of lead. Some very readable reflections of a general nature are put together in D'Arcy Thompson's "Growth and Form." We should not be impatient either of general reflections or of empirical equations; if properly pondered both will mightily help us forward. We are on our way. Whither we go we know not, but the way we must know.

The essential difference between an empirical equation and a law of nature is that the former is a description of our observations, whereas the latter involves induction from the observations. Now induction is the object of the experimental as contrasted with the observational method. A person who performs experiments merely for the sake of describing them seems unnecessarily to be shielding his eyes from the bright visions with which nature on every side surrounds him. We are content to take the limited controlled observation of the laboratory only because we have the greater aim of induction in view. Even though we expect that the induction will fall far short of that degree of universality which we ennoble with designation as a law of nature, we hope to elicit from the immediate occasion something which shall permit us to seize some part of its general significance, some iota of its real meaning. The significant realities of nature are nature's uniformities. At times and to individuals the problem of induction has seemed reasonably simple, a sort of correlative of deduction. Such opinions appear hardly tenable. I can not pursue the discussion here but may refer you to passages in Keynes's "Treatise on Probability" and Whitehead's "Science and the Modern World," and to my DeLamar lecture delivered two years ago at the Johns Hopkins University (Science, 63, 1926, pp. 289–296). My feeling is that unless one is willing to regard induction as a mere matter of chance, a lucky strike in the dark, he must believe it to be the original contribution of a mind lighted by an antecedent rationalism.

You asked me to speak of the statistical methods of treating data. I wish you had not. It is a mean subject. Those of you who have read the biography of the great Lord Rayleigh by his son will recall his statement that he does not believe in statistical methods, that the object of repeating an experiment is to judge of the control acquired, that he even doubts the utility of averaging values to obtain a mean, though he admits that this is carrying disbelief rather far. We find very little statistical analysis in experimental physics or chemistry to-day, a smaller relative amount, I think, than was found a generation ago; and even in astronomy, for which the method of least squares was developed by Gauss and in which it was universally applied in the past, there is a strong tendency to short-cut formal statistical processes. It is now to the biologist or economist that you must go for complicated statistical analysis. Why this state of affairs? May it perhaps lie in a contrast of the experimental and observational methods, in a difference of degree of attainable control? Shall we say

that when the control is good, when we are working in a field in which control is easy or when we are sufficiently astute or fortunate to design experiments so that those consequences in which we are interested are independent of the other variations, then we have no need of statistics and can go along with Lord Rayleigh? Shall we admit that statistics belongs rather in the field of observation and serves to replace control when that is not attainable or is repugnant to the nature of the investigator?

We must think here not so much of statistics as a method of description, but more of it as a basis for induction. If we believe that induction requires rationalism in experimental procedures which are relatively exact, we must feel that it requires an even higher degree of insight and thoughtfulness, of care and of diffidence when maneuvering in crude and complicated realms. It pays to begin the analysis with simple methods, to make diagrams, to rearrange the data and make new pictures, to dwell with the material until one knows its excellencies and its defects in respect to those particular items that may be important for the conclusions, to take into account not alone the data themselves but any general scientific considerations that may be germane to the induction. This attitude any one may cultivate; it is consonant with the admonitions of Bowley and of Westergaard to eschew elaborate processes which carry one out of touch with the original figures and which by their very elaboration give a false sense of security.

ACETIC ACID

EXPERIMENTS OF NEUMANN

		Temperature								
		78°	100°	110°	120°	130°	140°	150°	160°	185°
A	Pressure		393.5	411	432	455	477	498.5		565
	D. calc.		3.39	3.23	3.06	2.90	2.75	2.61		2.28
	D. obs.		3.44	3.31	3.14	2.97	2.82	2.68		2.36
	Exc. of D. obs.		+.05	+.08	+.08	+.07	+.07	+.07		+.08
B	Pressure		342.3	359.3	377.5	398.5	417.5	436.5		495
	D. calc.		3.35	3.18	3.02	2.85	2.70	2.57		2.26
	D. obs.		3.37	3.22	3.06	2.89	2.75	2.63		2.31
	Exc. of D. obs.		+.02	+.04	+.04	+.04	+.05	+.06		+.05
C	Pressure		258							382
	D. calc.		3.26							2.22
	D. obs.		3.17							2.25
	Exc. of D. obs.		−.09							+.03
D	Pressure		232		252	274	287.5	300		335
	D. calc.		3.23		2.87	2.72	2.58	2.46		2.21
	D. obs.		3.12		2.94	2.68	2.54	2.44		2.23
	Exc. of D. obs.		−.11		+.07	−.04	−.04	−.02		+.02
E	Pressure	164	186	197	209	221	232	243	253	269
	D. calc.	3.53	3.15	2.97	2.81	2.65	2.52	2.41	2.32	2.18
	D. obs.	3.41	3.06	2.91	2.75	2.61	2.50	2.40	2.31	2.22
	Exc. of D. obs.	−.12	−.09	−.06	−.06	−.04	−.02	−.01	−.01	+.04
F	Pressure	149	168			201				
	D. calc.	3.50	3.12			2.62				
	D. obs.	3.34	3.01			2.56				
	Exc. of D. obs.	−.16	−.11			−.06				
G	Pressure	137	156	166.5	180	188	199	208.2		230
	D. calc.	3.48	3.09	2.92	2.75	2.60	2.47	2.37		2.17
	D. obs.	3.26	2.98	2.81	2.61	2.50	2.40	2.29		2.14
	Exc. of D. obs.	−.22	−.11	−.11	−.14	−.10	−.07	−.08		−.03
H	Pressure	113	130	138.5	149	157.5	168.2	175		191.5
	D. calc.	3.42	3.03	2.85	2.69	2.55	2.43	2.33		2.15
	D. obs.	3.25	2.94	2.78	2.60	2.47	2.32	2.26		2.13
	Exc. of D. obs.	−.17	−.09	−.07	−.09	−.08	−.11	−.07		−.02
J	Pressure	80	92	98.5	106	112.5	117.3		129.2	
	D. calc.	3.32	2.91	2.73	2.58	2.45	2.35		2.21	
	D. obs.	3.06	2.76	2.61	2.46	2.34	2.27		2.11	
	Exc. of D. obs.	−.26	−.15	−.12	−.12	−.11	−.08		−.10	
K	Pressure	66	77.7	84	89.5	93	98	103		110.5
	D. calc.	3.26	2.85	2.68	2.53	2.40	2.31	2.24		2.12
	D. obs.	3.04	2.66	2.49	2.37	2.32	2.24	2.16		2.11
	Exc. of D. obs.	−.22	−.19	−.19	−.16	−.08	−.07	−.08		−.01

Except by accident, mathematical conclusions are no sounder than the premises that went into the formulation of the problem, and they may be much weaker because of those accessory assumptions which are so apt tacitly to creep in during the progress of the work. If resort must be had to the technical processes of correlation, it is well first to master the treatment of association in Yule's "Theory of Statistics," and it is important to bear in mind the words of R. A. Fisher in his excellent "Statistical Methods for Research Workers," namely:

> If we choose a group of social phenomena with no antecedent knowledge of the causation or absence of causation among them, then the calculation of correlation coefficients, total or partial, will not advance us a step towards evaluating the importance of the causes at work. . . . In no case can we judge whether or not it is profitable to eliminate a certain variate unless we know, or are willing to assume, a qualitative scheme of causation.

This means antecedent rationalism.

It may be instructive to illustrate the method of correlation on material from physics or physical chemistry, even though the physicist would not apply it. Willard Gibbs, in the course of his memoir on the equilibrium of heterogeneous substances, developed from theoretical considerations a formula for connecting the density *D*, pressure *p*, and absolute temperature *t* of a mixture of gases with convertible components, and tested it in a rough way on available data. Shortly thereafter in a paper on the vapor-densities of certain substances he returned to the matter in detail. His result was

$$\log \frac{D_1(D-D_1)}{(2D_1-D)^2} = -A - B \log t + \frac{C}{t} + \log p,$$

where D_1 is the density of the rarer component and may be computed from the molecular formula. This equation contains three constants A, B, C; but of these B is connected with the specific heats and is therefore not fittable in the empirical sense. He showed, however, that, for the cases he was treating, the term B log t could be neglected, provided its omission was compensated in the determination of A and C. The result for acetic acid was

$$\log \frac{2.073\ (D-2.073)}{(4.146-D)^2} = \frac{3520}{t_c+273} + \log p - 11.349$$

when the two constants $C = 3520$ and $A = 11.349$ were derived from the experiments of Cahours and Bineau, which he appears to have judged to be the best then attainable.

Calculations from this numerical equation were then compared by Gibbs with sixty-five experiments of Neumann over a wide range of temperatures and pressures. The result of the comparison is given in the table. There are divergences, systematic as well as accidental. The discussion which Gibbs gives to this matter is interesting. He can discuss it because he has a rational formula based on excellent determinations. If he had had merely an empirical formula fitted directly to these observations of Neumann, his discussion would have had to be changed, if indeed it could have been given at all. If we apply correlation, which is a method of least squares, to the figures of Neumann we shall inevitably weight most heavily those observations which tend to depart most from the linear regressions automatically set up by the method. This is proper if the departures are accidental; if they are due to systematic errors we shall be led off the track both of a correct rational explanation of the phenomenon in question and of a sound criticism of the observational material.

The tables necessary for the treatment of Neumann's data by the method of correlation are:

TABLE A

Pressure and Density

Pressure / Density	50 99	100 149	150 199	200 249	250 299	300 349	350 399	400 449	450 499	500 549	550 599	Totals
210–224	1	3	1	1	1	1						8
225–239	2	2	2	1	1		1		1		1	11
240–254	1	1	3	2	1	1						9
255–269	2	1	1	2	1			1	1			9
270–284	1	1	1	1				1	1			6
285–299		1	2		1		1		1			6
300–314	2		2	1			1	1				7
315–329		2			1		1					4
330–344		1	1			1	1	1				5
Totals	9	12	13	8	6	3	5	4	4	0	1	65

TABLE B

TEMPERATURE AND DENSITY

Temperature / Density	78	100	110	120	130	140	150	160	185	Totals
210–224						1	1	1	5	8
225–239				1	2	2	2	1	3	11
240–254			1	1	2	3	2			9
255–269		1	1	2	3		2			9
270–284		1	2	1		2				6
285–299		2	1	1	2					6
300–314	2	3		2						7
315–329	2	1	1							4
330–344	2	2	1							5
Totals	6	10	7	8	9	8	7	2	8	65

TABLE C

TEMPERATURE AND PRESSURE

Temperature / Pressure	78	100	110	120	130	140	150	160	185	Totals
50–99	2	2	2	1	1	1				9
100–149	3	1	1	2	1	1	1	1	1	12
150–199	1	3	2	1	2	2	1		1	13
200–249		1		1	2	1	2		1	8
250–299		1		1	1	1	1	1	1	6
300–349		1					1		1	3
350–399		1	1	1	1				1	5
400–449			1	1		1	1			4
450–499					1	1	1		1	4
500–549										0
550–599									1	1
Totals	6	10	7	8	9	8	7	2	8	65

The biometric constants computed from these tables are:

Mean density	= 2.682	Mean temperature	= 128.3	Mean pressure	= 229.6
Stand. Deviation: Density	= .375	S. D. temperature	= 30.0	S. D.: Pressure	= 126.8
S. D. of mean	= .046	S. D. of mean	= 3.7	S. D. of mean	= 15.6
S. D. of S. D.	= .33	S. D. of S. D.	= 2.6	S. D. of S. D.	= 10.8

The correlation coefficients between pairs of variables are

Pressure-Temperature	$\gamma_{23} = .38$	Density-pressure	$\gamma_{13} = .14$	Density-Temperature	$\gamma_{12} = . -.82$
Standard Deviation is	.10		.12		.04

This indicates no significant cogrediency of density and pressure, only a moderate cogrediency of pressure and temperature, but a very strong contragrediency of density and temperature. The partial correlation coefficients, however, tell a different story.

Correlation of pressure and temperature with density constant	$\gamma_{23\cdot1} =$	$.88 = .03$
Correlation of density and pressure with temperature constant	$\gamma_{13\cdot2} =$	$.87 \pm .03$
Correlation of density and temperature with pressure constant	$\gamma_{12\cdot3} = -$	$.95 \pm .01$
Standard deviation of density with pressure and temperature constant	$\sigma_{1\cdot23} =$	.105
Standard deviation of temperature with density and pressure constant	$\sigma_{2\cdot13} =$	9.0
Standard deviation of pressure with temperature and density constant	$\sigma_{3\cdot12} =$	58.

Regression equation of density on pressure and temperature

$$\frac{D-2.68}{.105} = .87\ \frac{p-127}{58} - .95\ \frac{t_c-128}{9.0}$$

Regression equation of pressure on density and temperature

$$\frac{p-127}{58} = .87 \ \frac{D-2.68}{.105} + .88 \ \frac{t_c-128}{9.0}$$

Regression equation of temperature on density and pressure

$$\frac{t_c-128}{9.0} = .88 \ \frac{p-127}{58} - .95 \ \frac{D-2.68}{.105}$$

(As the calculations are merely for illustrative purpose they have been run through with only slide rule precision and an accumulation of errors may appear in the final results. It is noticeable that $\sigma = .105$ for the root mean square residuals of the departures of the density as calculated from the regression equation fitted to the data happened to be the same value, estimated from the average departure .085, found by Gibbs when comparing his equation, fitted to other data, with this particular series.)

In getting forward with quantitative rationalistic science one of the chief purposes of expressing the experimental or observational results as an equation or formula is to have the relationship in such a form that it may be subjected to mathematical manipulation in combination with other formulas. It should be remarked that a regression equation is unhappily not subject to such manipulation. Even the simple process of solving a linear equation can not be carried out. If we should solve the second of the given regressions for D we should find:

$$\frac{D-2.68}{.105} = 1.15 \frac{p-127}{58} - 1.01 \frac{t_c-128}{9.0},$$

which is by no means identical with the first, the proper, linear expression for D, and will give results decidedly wide of the mark. This is awkward, but it is inevitable by virtue of the very nature of such an equation. As a matter of fact a regression is designed to give us the best average value of one variable, say D, when the precise values of the other variables are known. What the rationalist wants for his purposes is the best functional relationship between the variables all treated alike with respect to their different degrees of precision. For many purposes he prefers to know an appropriate type of function, not necessarily linear, than to emphasize any special numerical values, and for the discovery of functional types the method of partial correlation is awkward, to say the least. Such desires and preferences are coordinate with his aim of analyzing relations in an ideal system free from variations other than those of the variables on which he is concentrating his attention. I feel sure that many a physiologist of to-day feels this, even if he does not formulate it, and pursues his course in the manner of Galileo, of Newton, and of Helmholtz rather than after the style of Galton and Pearson.

Let it be clearly understood that I do not condemn the statistical method; in many cases it is indispensable. I come not to condemn, but to analyze. One has to admit that any method, by the very fact that it is a method, may tempt persons unintelligently to confide their fortunes to it to their own destruction (an observation hardly necessary here scarce fifty blocks from Wall Street). The rationalist who becomes too much idealist is peculiarly liable to defeat his own ends, to permit himself to be led too far afield by the imagined beauties of his own speculations, and never find his way back to nature. The illustrious investigator for whom this society is named said of Bacon that he wrote of science like a Lord Chancellor. That you may have less excuse to say of me that I have treated of your problems like a mathematical physicist, I wish at the close of this discourse to say a little about obtaining data; that is decidedly germane to our subject. I am much impressed with the great elaboration of experimental apparatus, especially in the hands of the young investigator. Such a condition is perhaps inevitable here in America, where we manufacture so many doctors of philosophy and where so many of these newer doctors enter on positions really as research assistants to maturer scientists. It is easy for the teacher or investigator to incorporate the candidate or the graduate into his own investigative system, technique and all, to start him with a material equipment comparable to that to which he himself has attained only after years of work and of thought. It is perhaps the selfish thing to do; in some cases it may be a necessary defensive action of the mature student if he is to have any further hope of progress with his own work. But the neophyte has not had those years of work and of reflection; he is not yet in intimate contact with the facts, the irreducible and stubborn facts, of nature. May not too elaborate equipment shield him from such contact? Might it not be better for him at first to perform more qualitative, fewer quantitative, experiments, to range around his field a bit and really become acquainted with it?

The inspiration to a fruitful scientific life comes from seeing nature not through some elaborate intermediary darkly as in a glass, but face to face. Galton's life is interesting not so much for its record of

his accomplishments as for the revelations of his freshness of mind. He was a great amateur, and such are the salt of the earth. It is their followers who systematize and reduce to method. Years ago, as I was wandering through the Jefferson Physical Laboratory in search of some former fellow student with whom to exchange ideas, I came upon my old teacher, B. O. Peirce. Said he: "Colonel Wilson, we are all poor physicists here in the Jefferson." "How so?" I asked. "Why, we have to have $14,000 a year to get our uninteresting results when a real physicist would get new stuff with a ball of twine and a jackknife!" Exaggeration, of course; nobody begrudges the Jefferson its budget, it is well spent; it should be larger. The adult must have his means of livelihood. My question regards the young. Is it well too urgently to transform the natural freely imaginative organization of the child's hide-and-seek into the supervised play of the school and the massed phalanx of the stadium under the direction of the professional coach? It is metaphor; but does it not somewhat apply to our conduct of graduate instruction and of initiation into a life of scientific research? Should we not distrust all over-elaboration of method, whether of obtaining or of treating data, whether of apparatus or mathematics or statistics? Howsoever inevitable such development is, should we not be actively on guard lest it lead us and particularly our youth inward to mooning over artificialities instead of out to live with the stubborn facts of a real world. If I knew a young fellow who sought advice about love I should not send him to his room to study Balzac's "Physiologie du Mariage" or Bourget's more ponderous "Physiologie de l'Amour moderne," nor yet to a clinic to be "psyched" à la Freud; I should tell him to go see some girls.

Edwin B. Wilson

School of Public Health of
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SIDNEY IRVING SMITH

Professor Sidney Irving Smith, Ph.B., Yale, 1867, was born February 18, 1843, in Norway, Maine, and died May 6, 1926, in New Haven, Conn. He had been in feeble health several years, due to various complications combined with his age. Immediate cause of his death was cancer of the throat. He had been partially blind since 1906, due to hereditary glaucoma. Although surgical operations were made on both eyes, he became totally blind several years ago. Professor Smith married in New Haven, June 29, 1882, Eugenia Pocahontas, daughter of Edward Brady Barber, a music-teacher from Canada. Mrs. Smith died March 14, 1916. There were no children. He is survived by a sister-in-law, Mrs. Clarence M. Smith, of Norway, Maine; his brother-in-law, Professor emeritus Addison E. Verrill (B.S. Harvard, 1862), M.A., Yale, 1867; Major George E. Verrill, '85 S. Yale, U. S. Engineer; A. Hyatt Verrill, *ex*-'91, Yale, Art, artist and author (nephews); Edith B., m. V. Akers; Lucy Lavinia, *ex.* Art, m. S. H. Howe, Jr. (nieces).

His preparatory training was received in the Norway Liberal Institute, and Bethel, Maine, Preparatory School. Professor Smith before coming to Yale had, under the instruction and encouragement of A. E. Verrill, collected and studied about all the flowering plants and ferns of Norway, Maine, and vicinity, discovering many rare species. He always retained his interest in botany and gardening. At the same time he made large collections of the local insects and found many undescribed species, some of them of great interest. His insects, obtained prior to 1864, were purchased by Professor Louis Agassiz for the Museum of Comparative Zoology. After that he collected insects for the Yale Museum. He joined Professor Verrill in various dredging expeditions in Long Island Sound and to the Bay of Fundy in 1864 to 1870, making collections for the Yale Museum, in which he had charge of the Crustacea for many years.

He was assistant in zoology at Yale, 1867–1874; instructor in comparative anatomy, 1874–75; professor of comparative anatomy, 1875–1906; and since then professor emeritus. He had charge of deep water dredging in Lake Superior for the United States Lake Survey in 1871, and for the United States Coast Survey about St. George's Banks in 1872; and was associated with the biological and deep-sea work of the United States Fish Commission, 1871–1887. Later, he gave his share of the deep-sea Crustacea to the Yale Museum.

He was state entomologist of Maine and Connecticut for a number of years and contributed to the annual reports of the Maine and Connecticut Boards of Agriculture. In 1890 he revised the definitions in comparative anatomy in Webster's International Dictionary. He organized and conducted one of the first, if not the first, biological course in this country of studies intended as a preparation for a medical school. He was an excellent teacher, using laboratory anatomical work extensively.

When the first Peabody Museum was planned, in 1875, he and Professor Verrill made all the plans and detailed drawings and specifications for the furniture and exhibition cases on the third floor, and part of those on the second floor of the museum. He was also one of the early promoters of the Biological Station at Woods Hole, Mass., and for several years one of its trustees. He had been a member of the National Academy of Sciences since 1884; and was also a member of various other scientific societies.

He was the author of numerous zoological papers

dealing especially with marine Crustacea, published in various scientific magazines, among them the *American Naturalist, American Journal of Science, Canadian Naturalist,* and *Annals and Magazine of Natural History* (London). In connection with his work for the U. S. Government he had contributed largely to the annual reports of the U. S. Fish Commission, Coast Survey, and Lake Survey, and also to reports of Department of Marine and Fisheries of Canada, of Committee on Progress of the Geological Survey of Canada, and the bulletins and proceedings of United States National Museum and Museum of Comparative Zoology.

A. E. VERRILL

SCIENTIFIC EVENTS

THE CONVERSAZIONE OF THE ROYAL SOCIETY[1]

THE Royal Society usually holds two conversaziones annually. The first of these gatherings was cancelled owing to the general strike, and the second was held on June 16 last. Many of the exhibits arranged in the society's rooms have already been referred to in our columns. Limitations of space forbid more than passing notice of some of the remainder.

The Department of Entomology, British Museum (Natural History) (Mr. F. W. Edwards and Dr. P. A. Buxton), exhibited specimens of a submarine Chironomid. This is the first insect species known to spend its whole life in the sea, and was discovered by Dr. P. A. Buxton in Samoa, where it was found associated with other Chironomid flies having normal air-living adults. Mr. T. S. P. Strangeways and Dr. H. B. Fell showed microscope preparations of the development *in vitro* of the isolated eye of the embryonic fowl. Fowl embryos of 64–70 hours' incubation were used. An eye is dissected out and explanted into a medium composed of fowl plasma and embryonic tissue extract; here it grows and differentiates in a surprisingly normal way.

The soil physics department, Rothamsted Experimental Station (Dr. B. A. Keen and Dr. W. B. Haines), exhibited a new combination of apparatus for measuring soil resistance which has marked advantages over the usual type of dynamometer. It is light and portable, and gives a continuous record of the draught, and a time scale, on a celluloid strip, which has great advantages over paper for outdoor work under varied weather conditions.

Lieutenant-Colonel F. J. M. Stratton and Mr. C. R. Davidson showed a number of photographs of the solar eclipse of January 14 last, taken at the expedition sent to Benkulen, Sumatra. The expedition obtained good photographs of the corona, the spectrum of the chromosphere and also of the corona both with slit spectroscope and prismatic cameras, from a discussion of which it is hoped that fresh knowledge may be gathered as to the condition and constitution of the solar atmosphere. From the spectrograms of the corona it will be possible to obtain accurate wavelength measurements of the lines of the coronal spectrum of unknown origin.

Professor O. W. Richardson showed an apparatus for the investigation of soft X-rays which are produced by the electronic bombardment of solids. The tube is of transparent silica, and is exhausted to a pressure of about one ten-millionth of a millimeter of mercury. The presence of the X-radiation is demonstrated by the photoelectric emission which it produces from a copper-plate enclosed in the tube.

The National Physical Laboratory had two interesting exhibits. The vector colorimeter (Mr. Guild and Dr. Perfect) enables a color to be specified by *qualitative* measurements involving color matches only. Two color matches are made. In one the test color is matched by a mixture of spectrum red with monochromatic light of suitable wave-length; in the other by a mixture of spectrum blue with another suitable monochromatic constituent. These matches determine the two vectors, and their intersection determined the position of the color on the color chart. A modified manometer for the determination of the vapor tensions of molten cadmium and zinc was also shown (Mr. C. H. M. Jenkins). This is measured by the pressure of nitrogen required to level the two liquid surfaces of the metal of a specially shaped manometer. The closed end of the manometer can be flooded by rotation of the apparatus, and foreign gases can also be removed by a similar rotation accompanied by a reduction of pressure.

Mr. W. M. Mordey showed some experiments demonstrating the possibility of getting a powerful rotation, in multiphase alternate current fields, of magnetic materials which are either non-conducting in themselves or are made so by being reduced to fine powder and then made up into solid discs or cylinders with some binding material, such as glue or shellac, which insulates the particles from one another. Such discs or cylinders, which form miniature induction rotors without windings and with non-conducting cores, can be made up of powdered magnetic materials such as hard cast-iron dust, hard steel grit, nickel, cobalt and of the magnetic minerals magnetite and pyrrhotite; in such bodies no appreciable eddy currents are produced, the rotation being due entirely to hysteresis. An interesting item in the exhibit was a small alternate-current electric fan, the rotor of which consisted simply of a piece of pyrrhotite rock

[1] From *Nature*.

with no winding on it, revolving freely in the field of a small model multiphase magnet. Loose powders of any of these materials rotated in such multiphase fields in a direction opposite to that in which solid masses of the same materials rotate, a reversal of direction due to a rolling action of the particles. No eddy currents are induced in any of the conducting materials when powdered. Dr. W. H. Eccles and Dr. Winifred Leyshon showed a neon tube and tuning fork combination for producing electrical oscillations of harmonic frequencies suitable for calibrating wavemeters. The neon tube is connected in parallel with a condenser and in series with a resistance and a battery; it then gives an intermittent luminous discharge and the current can be used to keep a steel tuning-fork in continuous vibration. The current in such a circuit has many high harmonics, and therefore induces an oscillatory current of any chosen harmonic frequency in a neighboring circuit tuned to that frequency.

Sir Robert Hadfield, Bart., exhibited a number of specimens of alloy steels for special purposes. These included a rotor in "ERA/ATV" steel used in the construction of exhaust gas turbines. These rotors are driven by the exhaust gases from internal combustion engines; they work continuously at a temperature of from about 800° to 950° C., and run up to the very high testing speed of 53,000 revolutions and working speeds of about 30,000 revolutions per minute. A tuning-fork of high nickel chromium alloy steel was shown which has constant frequency under varying temperature. For this purpose the metal must have a very low temperature coefficient of the modulus of elasticity.

THE RUSSIAN MINING CONGRESS

A REPORT of the proceedings of the first Federal Mining Technical Congress of the Soviet Union, held in Moscow, from April 13 to 27, and attended by 503 delegates, is made public by the Russian Information Bureau. The delegates included administrative and technical representatives of the various Soviet mining trusts, mineralogists and geologists of the Russian Academy of Sciences, officers of the labor organizations and representatives of the supreme economic council and other departments of the government.

All phases of the mining industry, including the oil industry, were discussed, including the latest technical methods and the training of engineers and technicians to direct the rapidly increasing output.

Representatives of the oil industry reported that the output would be increased from 8,500,000 metric tons during the present fiscal year, to 12,000,000 tons in 1929–30. Professor Taneyev pointed out that the Soviet Union contained 75 per cent. of the world's resources in peat, and the development of this fuel was of great importance to the electric generating plants and to the textile industry.

A report on recent explorations in the Urals cited important prospects for the mining of iron, copper, coal, gold, platinum, asbestos and other minerals in hitherto untouched territory. Professor P. P. Lazarev, chemist, and other members of the Academy of Sciences reported on extensive explorations and surveys undertaken by the academy to discover for exploitation new sources of mineral wealth. Plans were discussed for the production of arsenic, aluminum and the mining of graphite, tin and other metals not hitherto produced in the Soviet Union. Prospective plans for the development of the various branches of the mining industry were also fully discussed.

OPHTHALMOLOGICAL LABORATORY FOR HARVARD UNIVERSITY

A GIFT of $250,000 to Harvard University toward the foundation of an ophthalmological laboratory in memory of members of his family has been made by Dr. Lucien Howe, of Buffalo. Dr. Howe will become director of the laboratory when it is inaugurated next fall or winter.

The laboratory is expected to cost half a million dollars in all. Dr. Howe's donation will be supplemented by $175,000 from the General Education Board and $75,000 from Harvard University. The laboratory will serve for both research and teaching.

The institution will be officially called the Howe Laboratory of Ophthalmology, but Dr. Howe has explained that he would prefer to have it known as a memorial to his father, brother and father-in-law.

The father of Mrs. Howe commanded a division of the sixth corps in the Army of the Potomac in the Civil War, Dr. Howe's father commanded the 3d Cavalry, while his brother, a captain in the 4th Artillery, was killed in action early in life. These three men served in the regular army a total of over 70 years. It was therefore in accordance with the family plans to establish some memorial to that record which would seem lasting and also useful. The gift to Harvard University is the result.

The laboratory will be for some years to come in the present eye patient department of the Massachusetts Eye and Ear Infirmary, whose clinical opportunities are well known. A preliminary year will be given to forming a plan for its work, perhaps clearing up one corner of the field in physiological optics, or eye movements. It is hoped that the laboratory will in future become useful to research residents and to occasional workers at some special problem.

Dr. Howe was a student at Harvard in 1872 and 1873. He has been for nearly fifty years in charge of the Buffalo Eye, Ear and Throat Infirmary. For almost thirty years he has been professor of ophthalmology in the University of Buffalo.

RESEARCH BY FOREIGN SCHOLARS AT YALE UNIVERSITY

SIX foreign scholars will take up residence at Yale University next year to conduct research under the direction of the faculty of the Yale Graduate School. These include five research fellows of the International Education Board, Dr. Pasquale Pasquini, of the University of Rome, Italy; Dr. Stanislaw Hiller, of Cracow, Poland; Dr. Ernest Wolf, of the University of Heidelberg, Germany; Dr. B. M. Bergerson, of the University of Oslo, Norway, and Dr. Fritz E. Lehmann, of the University of Freiburg, Germany, who have been appointed to carry on research under the direction of Dr. Ross G. Harrison, of the department of zoology, and Dr. D. L. Watson, of Edinburgh, Scotland, who will work in physics.

In addition to these five foreign research fellows, ten other fellows have been appointed by the National Research Council and the National Tuberculosis Association to study specific problems under the direction of the graduate faculty. Nine of these assignments are made by the National Research Council as follows: Franklin Hollander, Ph.D., Columbia University, 1923, of Brooklyn, N. Y.; Olive M. McCay, Ph.D., University of California, 1925, of Berkeley, Calif.; Leopold R. Cerecedo, Ph.D., University of Freiburg, 1921, of San Juan, Porto Rico, and Howard J. Shaughnessy, B.S., Massachusetts Agricultural College, 1922, of New Haven, Conn., have been granted fellowships by the medical board of the council to continue work with Professor Lafayette B. Mendel, of the department of physiological chemistry. Dr. A. J. Gee, of the University of Toronto, who is a National Research Council fellow, will take up his work under the direction of the department of bacteriology. On similar appointments Ernest O. Lawrence, Ph.D., Yale University, 1925, of Springfield, South Dakota, and Dr. J. W. Beams, of the University of Virginia, will conduct their research under the direction of the department of physics. In addition the child development committee of the National Research Council has appointed as fellows Miss Edith Fisher Symmes, chief psychologist of the Boston Psychopathic Hospital, and Miss Viola May Jones, assistant superintendent of the child-placing department of the State Charities Aid Association of New York City to work under the direction of Professor Arnold Gesell, of the Yale psycho-clinic.

As a fellow of the National Tuberculosis Association, Robert DeWolf Coghill, Ph.D., Yale University, 1924, will continue his research on the tubercle bacilli under the direction of Professor Treat B. Johnson, of the department of chemistry.

It is expected that the facilities of the university will be used by a number of visiting members of the faculties of other institutions. Professor C. C. Chen, of Shanghai College, will undertake special research in bacteriology, and Professor Arthur T. Jones, of Smith College, and Professor Mildred Allen, of Mount Holyoke College, in physics.

BIOLOGICAL ABSTRACTS

IN his review of the activities of the Rockefeller Foundation for the last year, President George E. Vincent says concerning biology:

> One special form of aid to the progress of biology calls for separate notice. The enormous number of scientific papers and volumes published annually through the world in every field of research creates the need for some kind of systematic organization of this material in a readily accessible form. A great library undertakes at least a part of this task. Such an institution has been likened to a social memory or brain.
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> But each library after all is only a section of a national and of a world memory or brain. The books and periodicals on its shelves come from all lands where new truth is discovered and then described in print. Thus in the field of biology alone it is estimated that each year 40,000 articles of at least some value appear in 5,000 journals, transactions of scientific societies, proceedings of congresses and the like. To be sure, these papers vary enormously in their importance. Probably in a given year only a small percentage is highly significant in fundamental ways. The bulk of them perhaps deal with useful details. A good many are likely to be trivial if not negligible. Yet if a scientific worker is to avoid duplicating the research of others, if he is to compare his methods with theirs, if he is to have his mind steadily fertilized by relevant ideas and suggestions, if he is to increase the chance of getting a happy illuminating flash upon his problems, he must have constant access to the world memory.
>
> To meet this need abstract journals which give the gist of articles and papers have appeared in different countries. Elaborate indexes make reference easy and accurate. Some of the journals have attained international standing. Biology as a whole, however, has lacked satisfactory service of a world-wide sort. Recently eighteen American biological societies joined in a plan to publish a journal of biological abstracts on an international basis. The cooperation of individuals and organizations in foreign countries is being sought and in most cases secured. The National Research Councils of Japan and of Australia have responded warmly. The Royal Society of London and the French Federation of Natural Science Societies have expressed an interest. Arrangements for exchange of material with abstract journals in Europe are being worked out.
>
> The details of the new plan have been carefully studied.

Eight sections of subject matter will be handled by eighty special editors. Cooperating foreign correspondents and libraries will help to scan the 5,000 serials for significant papers. Authors and editorial collaborators will prepare the abstracts. By the use of small but legible type and a thin opaque paper 1,030 large pages will occupy only an inch of shelf room. When once under way it is expected that twelve monthly numbers with elaborate annual indexes will run to between 3,000 and 3,500 pages. The entire enterprise will be directed by a small full-time central staff of editors.

THE FOURTH AMERICAN ASSOCIATION PRIZE

THE thousand-dollar prize that is to be awarded at the fifth Philadelphia meeting of the American Association for the Advancement of Science will be the fourth annual prize of the association. The prize is awarded each year to some one who presents at the annual meeting of the association and associated organizations a paper making a noteworthy contribution to the advancement of science. There is no formal competition for the prize and all papers on the program are to be considered. The winner of the prize need not necessarily be a member of the American Association nor of any of the associated organizations. The prize is awarded at the close of the annual meeting and disbursement from the treasury is made within about a week thereafter.

Funds for these annual American Association prizes were given to the association by one of its members who wishes his name withheld. There is now available sufficient money for two more prizes after the one to be awarded at Philadelphia. These will be awarded at the Nashville meeting (1927–28) and at the fifth New York meeting (1928–29).

The following is the roll of the American Association prizemen. The Washington prize was divided equally between two winners.

1. Dr. L. E. Dickson, professor of mathematics in the University of Chicago. Awarded the first American Association prize, at Cincinnati, January, 1924, for a noteworthy contribution on "Algebras and their Arithmetics." (See SCIENCE for January 25, 1924, p. 77.)

2a. Dr. L. R. Cleveland, research worker in medical zoology at the School of Hygiene and Public Health, the Johns Hopkins University. Awarded half of the second American Association prize, at Washington, January, 1925, for noteworthy contributions on "The Ability of Termites to Live Perhaps Indefinitely on a Diet of Pure Cellulose" and "The Effects of Starvation and Oxygenation on the Symbiosis between Termites and their Intestinal Protozoa, together with the Toxicity of Oxygen for Free-living and Parasitic Protozoa." (See SCIENCE for March 13, 1925, pp. 277–279.)

2b. Dr. Edwin P. Hubble, astronomer at Mt. Wilson Observatory, Pasadena. Awarded half of the second American Association prize, at Washington, January, 1925, for a noteworthy contribution on "Cepheids in Spiral Nebulae." (See SCIENCE for March 13, 1925, pp. 277–279.)

3. Dr. Dayton C. Miller, professor of physics in the Case School of Applied Science, Cleveland. Awarded the third American Association prize, at Kansas City, January, 1926, for a noteworthy contribution on "The Michelson-Morley Ether-Drift Experiment: its History and Significance." (See SCIENCE for January 29, 1926, pp. 105–106, and for April 30, 1926, pp. 433–443.)

By vote of the association council, the annual prizes are not to be divided in the future, the entire amount of the prize going each year to a single winner. The award is decided by a special committee on prize award. This committee for the fifth Philadelphia meeting has been named as follows:

C. E. Seashore, *chairman;* dean of the Graduate College, University of Iowa.

Otis W. Caldwell; director of the Lincoln School, Teachers College, New York City.

Charles B. Davenport; director of the Station for Experimental Evolution, Cold Spring Harbor, N. Y.

Lauder W. Jones; professor of chemistry, Princeton University.

C. F. Marbut; chief of the Division of Soil Survey, Bureau of Soils, U. S. Department of Agriculture.

The committee receives suggestions from the secretaries of the sections and societies that take part in the general program of the meeting, studies these suggestions and reports its decision to the permanent secretary, who announces the award through the daily and scientific press at the close of the annual meeting.

BURTON E. LIVINGSTON,
Permanent Secretary

SCIENTIFIC NOTES AND NEWS

THE degree of doctor of science has been conferred by the University of Oxford on Professor A. C. Seward, professor of botany, master of Downing College and vice-chancellor of the University of Cambridge; Sir William Bragg, Fullerian professor of chemistry and director of the Davy Faraday Research Laboratory of the Royal Institution, London, and Sir Walter Morley Fletcher, secretary of the British Medical Research Council.

THE degree of doctor of science will be conferred in October by the University of Cambridge on Sir Ernest Rutherford and on Professor W. S. Holdsworth on the occasion of the celebration by Trinity College of the Bacon Tercentenary.

THE bicentenary of the faculty of medicine of the University of Edinburgh was celebrated on June 10

and 11. At the special graduation ceremonial the honorary degree of LL.D. was conferred on the following alumni of Edinburgh: Dr. Andrew Balfour, director of the London School of Hygiene and Tropical Medicine; Professor Robert Howden, professor of anatomy, University of Durham; Professor W. T. A. Jolly, professor of physiology, University of Cape Town; Sir George Newman, Ministry of Health; Professor Alexander Primrose, professor of clinical medicine, University of Toronto; Sir John Robertson, professor of public health, University of Birmingham; Professor Ralph Stockman, professor of materia medica, University of Glasgow; Dr. A. Logan Turner, president of the Royal College of Surgeons, Edinburgh; Sir Norman Walker, treasurer of the Royal College of Physicians, Edinburgh; Professor J. T. Wilson, professor of anatomy, University of Cambridge. Sir George Newman made the principal address.

THE degree of doctor of science was conferred on June 14 by Iowa State College on Matthew Luckiesh, director of the Nela Research Laboratories, Cleveland, Ohio.

DR. FRANKLIN H. MARTIN received the honorary degree of doctor of public health at the recent graduating exercises of the Detroit College of Medicine and Surgery.

DR. EDWARD J. MENGE, head of the department of biology at Marquette University, Milwaukee, has been awarded the honorary degree of doctor of science by De Paul University, Chicago.

CHARLES EMBREE THORNE, former director of the Ohio Agricultural Experiment Station and at present consulting chief in agronomy, has received the degree of doctor of science from the College of Wooster, Ohio.

DR. J. CLARENCE WEBSTER, Shediac, N. B., formerly professor and head of the department of obstetrics and gynecology at Rush Medical College, received the honorary degree of doctor of laws at the recent commencement exercises of Dalhousie University, in recognition of his work in forwarding the interests of education and of his research in the history of the maritime provinces.

SIR HUMPHRY ROLLESTON, Bart., Regius professor of physic in the University of Cambridge and lately president of the Royal College of Physicians, has been awarded the gold medal of the British Medical Association.

THE Paris Academy of Sciences has given the Monaco prize to Dr. Jean Charcot for his scientific expeditions and to help him in a new one he is undertaking in the *Pourquoi-Pas*.

PROFESSOR J. FIBIGER, of Copenhagen, was tendered recently a complimentary dinner by the staff of the Cancer Hospital at London.

NOMINATIONS for election in September for president and vice-presidents of the American Society of Mechanical Engineers are: for president, Charles M. Schwab, chairman of the Bethlehem Steel Corp., New York, N. Y.; vice-presidents, Charles L. Newcomb, manager, Deane Works, Worthington Pump Corporation, Holyoke, Mass.; E. O. Eastwood, professor of mechanical engineering, University of Washington, Seattle, Wash.; Edwards R. Fish, vice-president of the Heine Boiler Co., St. Louis, Mo.

IN order to resume his work as dean of the College of Agriculture and director of the experiment station of the University of Kentucky, Thomas P. Cooper, chief of the bureau of agricultural economics, has tendered his resignation. Lloyd S. Tenny, assistant chief, has been appointed acting chief of the bureau.

DR. B. M. DUGGAR, professor of plant physiology at the University of Missouri and physiologist to the Missouri Botanical Garden, has been elected a member and chairman *pro-tem.* of the board of trustees of the Bermuda Biological Station.

PROFESSOR LEON A. HAUSMAN, of Rutgers University, has recently been made the science editor of *Compton's Pictured Newspaper* of Chicago. He has also been invited to contribute studies of mammal hair and fur to the revised edition of the Encyclopaedia Britannica.

DR. FRANCIS L. SIMONS, instructor in chemistry at Brown University, has joined the research staff of Skinner, Sherman and Esselen, Inc., of Boston.

THE British secretary of state for the Colonies has appointed Dr. A. T. Stanton to be his chief medical adviser. Since 1921 Dr. Stanton has held the post of director of government laboratories in the Federated Malay States.

THE *Journal* of the American Medical Association states that Dr. John D. Long, formerly assistant surgeon general of the U. S. Public Health Service, has tendered his resignation as technical adviser in public health to the ministry of hygiene of Chile. Dr. Long was lent by the U. S. government to reorganize the national health service in Chile. According to the Chicago *Daily News,* his efforts to reorganize the health service of Chile were blocked by politics of the Figueroa cabinet and the opposition of the property owning class, who were forced to make their tenements livable and sanitary.

DR. THOMAS A. JAGGAR, volcanologist of the U. S. Geological Survey, who is in charge of the investiga-

tions which have been made from Volcano House on the side of Mauna Loa in Hawaii has undertaken to make a comprehensive scientific study of Lassen Peak in Lassen National Park, to cover several months of observation.

DR. A. S. HITCHCOCK has left Washington for the Rocky Mountains, where he will carry on further studies in grasses, especially our native blue-grasses, for his Manual of Grasses of the United States now in preparation. Beginning at the south he will visit regions not previously investigated in New Mexico, Colorado, Wyoming and Montana.

DR. CASEY A. WOOD has spent a year in Ceylon collecting material on comparative vision in birds and has now gone to Kashmir to continue this study for six months, when he will return to Chicago by way of China and the Philippine Islands; Dr. Wood has been made honorary collaborator in the Division of Birds, Smithsonian Institution, Washington, D. C., for his contributions to ornithology.

CHARLES T. GREENE, of the Bureau of Entomology, has returned from a four months' stay in the Canal Zone, where, in cooperation with the Federal Horticultural Board, he has been investigating fruit flies.

RALPH A. SAWYER, assistant professor of physics at the University of Michigan, who was awarded a John Simon Guggenheim Memorial Fellowship for the coming year, will make a study of spectral series relations in extreme ultra-violet metallic spectra and the correlation of the results with modern theories of atomic structure. He will work principally in the laboratory of Professor F. Paschen, president of the Imperial Physico-Technical Institute, Charlottenburg, Germany.

DR. WILLIAM H. HOOVER, who has been in charge of the solar observatory of the Smithsonian Institution, at La Quiaca, since 1924, reached Washington on June 26. He will proceed to Mount Brukkaros, Southwest Africa, where he will direct the work of the new observatory in the Eastern Hemisphere recently established by the Smithsonian's solar expedition under the direction of Dr. C. G. Abbot.

DR. ROBERT BALLENEGGER, head of the College of Horticulture, at Budapest, is working in the department of soils in the Michigan Agricultural College, East Lansing, as exchange professor under the American-Hungarian Foundation.

D. MORLAND, apiarist of the Rothamsted Experimental Station, England, has visited Washington to spend some time at the bee culture laboratory of the Bureau of Entomology. He is also visiting Ithaca and western points of interest to advanced beekeepers.

DR. DOUGLAS FRYER, of New York University, left on July 1 for Europe, where he will make a study of psychological conditions.

DR. O. W. H. MITCHELL, professor of bacteriology, hygiene and sanitation in the college of medicine of Syracuse University, has been granted a year's leave of absence which he will spend in traveling and studying in Europe.

C. B. JORDAN, dean of the school of pharmacy, Purdue University, has been granted a year's leave of absence which he will spend at Cambridge, Mass. He expects to carry on some work in colloid chemistry and physiological chemistry at Harvard University.

DURING the summer session at Columbia University, a series of special lectures on contemporary developments in chemistry will be given by a number of well-known authorities, including M. T. Bogert, J. F. Norris, P. A. Levene, W. M. Clark, R. W. G. Wyckoff, Irving Langmuir, H. T. Taylor, Sir James C. Irvine, E. C. Kendall, H. G. Wells, R. B. Moore, E. E. Reid, Leonor Michaelis, C. A. Kraus, Ernst Cohen, J. A. Wilson, J. C. W. Frazer, R. B. Johnson, W. D. Bancroft, E. V. McCollum, Harry Steenbock, J. N. Brönsted, C. G. Fink, E. C. Franklin, C. A. Browne and Peter Debye. The lectures began on July 6 and will end August 12.

THE annual oration of the London Dermatological Society was delivered on June 16 by Sir Humphry Rolleston, who selected as his subject, "The Relations of Dermatology and General Medicine."

Nature records the death of Professor A. Magnin, formerly professor of botany and director of the Botanic Garden, University of Besançon; of Professor W. F. Shanks, professor of physiology in the University of Leeds since 1923 and formerly lecturer in physiology in the University of Glasgow, and of Professor V. A. Steklov, professor of mathematics at the University of Leningrad and vice-president of the U. S. S. R. Academy of Sciences.

THE *Journal* of the American Medical Association reports that the first congress of the Czechoslovakian Society for Roentgenology and Radiology was held in Prague, May 22–24, under the presidency of Professor R. Jedlička, of Prague. Radium was first isolated from the uranium ore of Jachymov, Bohemia. The congress held its sessions in four sections. The first section dealt with roentgenologic diagnosis; the second, with the physics and technic of radiology; the third, with biologic problems of radiology and roentgenology, and the fourth, with treatment of pathologic conditions by the means of roentgen rays and radium. The paper by the chief of the department of physics at the Institute of Radiology, Dr. F. Behounek, was read by title, because, at the time of the congress, he

was participating in Amundsen's expedition to the North Pole.

Nature states that further details of Miss Garrod's discovery of a skull, presumably of Mousterian age, at Gibraltar, tend to confirm the first impressions of its importance. In addition to the frontal bone another large piece of bone was found, but owing to the hardness of the matrix in which it is imbedded it is impossible yet to say whether it is the parietal or the occipital. Decision on this and other points must wait until it has been cleared.

UNIVERSITY AND EDUCATIONAL NOTES

Charles Hayden, a New York banker, retiring president of the Alumni Association of the Massachusetts Institute of Technology, has given $100,000 toward the dormitories to be erected at the institute, planned to accommodate 800 students.

Dean Arthur M. Green, Jr., of the engineering school at Princeton University, has announced plans for a proposed half-million-dollar engineering building. The Princeton Engineering Alumni Association is raising an additional $350,000 to equip the building. The new building will be of stone, three stories high, with a basement for service rooms, and will provide 70,000 square feet of floor space. It will house the mechanical engineering laboratory, the electrical laboratory, the hydraulic laboratory and the machine shop.

Dr. William Benjamin Smith, professor of physics in the University of Missouri, 1885–88, and professor of mathematics, 1888–93, has given to the university library 325 volumes in the field of mathematics and physics; 300 volumes in philosophy; 900 volumes on theology and religion, and 525 volumes on history, literature and the world war. Dr. Smith's library includes numerous rare books.

Lafayette College has appointed Freeman Ward, Ph.D., professor of geology at the University of South Dakota and state geologist of South Dakota, to the chair of geology vacant by the death of the late Professor Peck.

At Brown University, Dr. Mark H. Ingraham, of the University of Wisconsin, has been appointed assistant professor of mathematics, and Paul N. Kistler, of Lehigh University, assistant professor of mechanical engineering.

Dr. David H. Bergey has been promoted to an assistant professorship of hygiene and bacteriology in the medical school of the University of Pennsylvania. Dr. Morton McCutcheon has been appointed assistant professor of pathology.

The following promotions will take effect at George Washington University next autumn: Dr. Edwin A. Hill, to be professor of chemistry; Arthur F. Johnson, to be associate professor of mechanical drawing; Norman B. Ames, to be associate professor of electrical engineering, and Paul H. Brattain, to be assistant professor of chemical engineering.

Dr. W. E. H. Berwick, reader in mathematics in the University of Leeds, has been appointed to the chair of mathematics at the University College of North Wales, Bangor.

Arthur Hutchinson, F.R.S., fellow of Pembroke College, University of Cambridge, has been elected to the professorship of mineralogy, in succession to the late Professor W. J. Lewis.

DISCUSSION

THE STRUCTURE AND FORMATION OF BAST FIBERS IN FLAX

Microchemical work upon the history of bast fiber wall development in flax (Linum usitatissimum) has led to a somewhat different conception of cell wall formation and to the discovery of some interesting facts concerning cell wall structure. Bast fiber cells are distinguishable in the stem tip about the time that the vascular elements are clearly differentiated. The primary walls consist of cellulose containing pectose in the region of the middle lamella. Subsequent additions to this membrane are not continuous and gradual but by successive deposits of definite cellulose lamellae. The lamellae first appear in a gelatinous and much infolded condition, out of contact with the existing wall. Each lamella is pushed against the already existing cell wall, where its gelatinous consistency permits it to be closely fitted. In this position it loses its gelatinous consistency and becomes a part of the wall itself. The wall of the bast fiber then is formed by periodic deposits of cellulose lamellae. There is no cementing material between the lamellae, and by the use of suitable reagents the layers may be readily separated from each other, even in mature fibers. The zonation visible in the cross-section of the bast fiber results from these periodic deposits. The fine dark lines do not represent actual material, as has been supposed, but are merely boundaries between the successive lamellae.

When swollen with concentrated sulfuric acid and subjected to pressure while in the swollen condition, the lamellae reveal their basic structure. Each lamella is formed of spirally wound fibrillae and the direction of the spiral is reversed in each successive lamella. The fibrillae give parallel extinction under polarized light and have a high birefringence. The fibrillae are therefore crystalline. The bast fibers of

flax must be regarded as aggregates of innumerable spirally wound crystalline fibrillae, the spirals being alternately right and left handed in successive lamellae.

The alternate layers of right and left hand spirals may be due to different forms of isomeric celluloses. A lamella with right-hand spirals may possibly be the result of the condensation and crystallization of a dextro form. This may leave the laevo forms in excess and so may influence the formation of more laevo forms. These on reaching the saturation point may crystallize out as a definite lamella of laevo crystals, *i. e.,* a lamella with left-hand spirals, and leave the dextro forms once more in excess. A continuation of this process would account for the alternation of right and left hand spirals in consecutive lamellae. It may also explain the sudden and periodic deposits of cellulose lamellae. Work with the aim of establishing or discrediting this hypothesis is now under way.

This much is known:

(1) Flax bast fibers are formed by the periodic addition of definite lamellae of pure cellulose to the existing walls and not by a continuous and gradual acquisition of cellulose particles.

(2) The lamellae are deposited out of contact with the existing wall in a much infolded, gelatinous condition and are subsequently pushed to the walls.

(3) There is no cementing material between the lamellae and they may readily be separated with proper treatment.

(4) The lamellae are composed of spirally wound crystalline fibrillae.

(5) Consecutive lamellae have spirals in opposite directions. This may be the result of the presence of isomeric forms of cellulose.

(6) These facts have an important bearing upon the elasticity, permeability, strength, durability and adsorptive powers of such fibers.

Donald B. Anderson
The Department of Botany,
Ohio State University

SHOULD NEW FOSSIL SPECIES BE DESCRIBED FROM WELLS?

Subsurface correlation work in various oil fields has brought to light a number of species of microscopic fossils that are new to science. There has been much informal discussion of the question which this paper now places before paleontologists—that is, should fossils found in wells be described as new species and published as such in recognized journals?

There are certain arguments against describing new species from wells. Chief among these is the impossibility of obtaining topotypes from "type localities." This was pointed out by Joseph A. Cushman when he stated the following:

> It is a rule of paleontology that new species should not be described from well borings because of the uncertainty of depth and the impossibility of giving a type locality from which future collections may be made.[1]

With improved methods of drilling, should this rule of paleontology be abandoned? If it is, how would future collectors obtain topotypes from type localities? Of course, a new rule might be established requiring that authors describing new species from wells deposit specimens, with complete data as to locality, in some recognized institution, such as the United States National Museum, or in several institutions prepared to care for type specimens. This method would overcome the objection to indefinite locations such as that in a paper by Frederick Chapman,[2] where a species is described "from a well in Santa Clara County."

There are a number of reasons in favor of so describing such fossils. The foremost of these is that a well sample is just as accurately located as a dredged sample, except insofar as age is concerned. Moreover, some formations are discovered in wells that are not present locally at the surface, as in the case of certain areas in Texas, Louisiana, Oklahoma and elsewhere. The description of new species from wells in these localities might aid in subsurface correlation. G. Dallas Hanna and E. G. Gaylord, with this in mind, described *Scalez petrolia* from the Midlands Oil Company's Well No. 1, San Joaquin Valley, California.[3] This fossil, according to Dr. Hanna,[4] has turned out to be an especially valuable "marker" in economic work. Esther Richards Applin doubtless adopted the same point of view when she described several new species and varieties of foraminifera from wells in the coastal plain of Texas and Louisiana.[5]

Even with the precautions mentioned above, should this procedure be recommended? The consensus of opinion of the west coast paleontologists who have been consulted in this connection seems to be in favor of describing new species from wells, for, as Pro-

1 "Foraminifera from the Deep Wells of Florida," 13th Annual Report, Florida Geological Survey, p. 23, 1921.

2 Chapman, Frederick, "Foraminifera from the Tertiary of California," Proc. Calif. Acad. Sci., 3d ser. geol., Vol. 8, 1900.

3 "Description of a New Genus and Species of Freshwater Gastropod Mollusk (*Scalez petrolia*) from the Etchegoin Pliocene of California," Proc. Calif. Acad. Sci., 4th ser. Vol. 13, No. 9, pp. 147–149, March 18, 1924.

4 Personal communication, January 21, 1926.

5 In Applin, Ellisor and Kniker, "Subsurface Stratigraphy of the Coastal Plain of Texas and Louisiana." Bull. Amer. Assoc. Pet. Geol., Vol. 9, No. 1, 1925, pp. 79–122.

fessor James Perrin Smith has remarked to the writer, "A new species is a new species, no matter where it is found."

Hubert G. Schenck
Stanford University

THE PRICE OF HONOR

"Ph.D." 's letter in Science for April 16 moves me to quote a letter written me on May 28, 1925, from this same Professor Fritz Holm, G.D.G., LL.D., D.C.L., D.Lit., Chamberlain to His Royal Highness the Count of Caserta (Prince Alphonse of Bourbon-Sicily, Head of the Royal House of Naples).

On that occasion His Excellency's secretary informed me that His Excellency (then residing in Paris) had "read with interest and admiration of your Science League to combat silly attitudes by lawgivers and others towards questions of evolution, etc. His Excellency wishes me to inform you, that you may use his name in the literature of your league in capacity of honorary vice-president or simply vice-president."

I replied that the officers of the Science League of America were elected, that only members were eligible to office, and that membership was $3 a year. No reply was vouchsafed me. Evidently the "dollars (or bills)" are all supposed to flow in one direction!

Maynard Shipley,
President, Science League of America

QUOTATIONS

CHEMISTRY AND DISEASE

Dr. Charles H. Herty recently pointed out that we spent annually $1,015,000,000 to keep our 115,000,000 bodies in repair, as follows:

Drugs, including patent medicines	$ 500,000,000
Doctors' services (estimated on basis of average income per doctor per year of $1,500)	220,000,000
5% interest on the $624,000,000 of hospital investments in lands, buildings and furnishings	31,000,000
Hospital maintenance	264,000,000
	$1,015,000,000

In commenting on this, Senator Ransdell, of Louisiana, in a speech supporting a bill providing for the appropriation of $20,000,000 for the study of the cause, prevention and cure of disease, asked whether it would not be worth while to spend a few millions a year in order to determine whether this vast bill of a billion could not be reduced. Much has been done privately, notably by the Rockefeller Institute for Medical Research, the Carnegie Institution of Washington and other institutes and laboratories. But in most of these institutions comparatively little time is allowed for concentrated work on problems of major importance, or opportunity given for cooperative effort of the chemist, the biologist, the pharmacologist, the therapeutist and the physiologist.

Senator Ransdell's bill, which he "hopes will be favorably acted upon at the next session of Congress," contemplates the enlargement of the Hygienic Laboratory of the Public Health Service into a chemo-medical research laboratory. Specifically, it provides for an appropriation of $2,000,000 a year for five years for this enlargement, and in addition $10,000,000 to establish an academy of health in the District of Columbia or its vicinity. In such an institution a joint attack may be made on fundamental problems of medicine by leaders in chemistry, physics, biology, pharmacology and medicine, just as, out in California, physicist, astronomer and chemist have brigaded their efforts in an attack upon the forces of the atom. A similar coordination of effort was made by scientists in search for poisonous gas during the war.

Research service in conservation of the health of the nation should not be left entirely to private interest, however generous, zealous and intelligent. Particularly is it desirable that chemistry should be brought back, in its highest development as a science, to the aid of the physician in the prevention of disease and the alleviation of suffering. It has turned its attention in recent decades mainly to the production of wealth in the industries. It has a higher ministry before it if it can be brought to cope with disease in time of peace, as its aid was invoked by the government for destruction during the war. We have gone further in our federal departments in concern for the health of the lower animals, and even of trees and plants, than we have for that of human beings.—The New York *Times*.

SCIENTIFIC BOOKS

A History of British Earthquakes. By Charles Davison. Cambridge, at the University Press, 1924.

The authentic history of British earthquakes begins, according to the author of this accurate chronicle, with the year 974 A. D. Earlier occurrences, whose reality can hardly be doubted, but whose dates and places are not identifiable and whose character may be open to suspicion, are classed as legendary. We have here the keynote to Dr. Davison's work. It is scrupulously precise. Possibly mistaken or spurious records are rigidly excluded from his accounts, which nevertheless include 1,191 shocks in 950 years, 974 to 1924.

Geographical limitations are equally definite. The

scene does not cover the platform of the British Isles, which might be considered a geologic unit, but is restricted to the lands of England and Scotland. For this area there is a list, in which each one of the 1,191 shocks is given an arbitrary, sequential number and is cited by date, hour, center, intensity when known and area of disturbance. Ireland, the Channel Islands and the Shetland Islands are treated in a separate chapter under the heading "Extra-British Earthquakes." This also includes a description of the effects of the Lisbon earthquake so far as they were recorded in England.

The citations which constitute the greater part of the work are arranged by localities and proceed from Scotland through Wales and the Midland counties to southern and southeastern England. Of the space allotted to the descriptions of shocks, two thirds are occupied by those of Scotland and Wales, an indication of the greater seismic activity of those regions as compared with England proper. It is also a fact that the northern country is more rugged and of more complex geologic structure, more distinctly faulted.

The geographic extent of British earthquakes is usually small. The author has defined the disturbed area, wherever possible, meaning thereby the area within which the shock was even slightly perceptible. The facts are determinable, as a rule, only for shocks of the last sixty years and not for a large proportion of them; but the figures are sufficiently numerous to indicate that those which have even slightly disturbed as much as fifty thousand square miles are few. It may be inferred that the depths of the foci are comparatively moderate and the intensities not very high.

The chapter headings indicate the centers of greatest activity: Inverness, near the Great Glenn fault; Comrie and Menstrie, near the Highland Border fault; north Wales and south Wales; and many districts scattered throughout England are all distinguished. The author links each grouping of successive shocks with a known or supposititious fault. He thus suggests that the British Isles, like California, are traversed by active dislocations, although no visible displacements have been observed at the surface. To one who knows both countries the differences in topographic expression and geologic history appear so marked that they raise a doubt as to whether the mechanics of the earthquakes are necessarily similar.

Destructive earthquakes have been few in Britain. The writer enumerates eleven in England, five in Scotland, and two in Wales. Most of them, however, did little more than throw down chimneys. The most severe disturbances did damage massive stone structures, whose great weight rendered them liable in spite of the thickness of the walls. We quote:

1180, about September 29, an earthquake fatal to many great buildings in England, especially to Lincoln church. (Mallet)

On the mondaie in the weeke before Easter, chanced a sore earthquake through all parts of this land, such a one as the like had not beene heard of sithens the beginning of the world. For stones that laie couched in the earth, were remoued out of their places, stone, houses were overthrowne, and the great church of Lincoln was rent from the top downwards. (Holinshed)

1580, April 6. A very great earthquake in London and almost generally throughout England. Stukely says that no houses were thrown down. A piece of Temple Church, however, fell, also some stones from St. Pauls Church, a stone fell from the top of Christ Church, which killed an apprentice, and various chimneys were thrown down. At Dover, a part of the cliff fell into the sea with a portion of the castle wall. A small part of Saltwood Castle and of Sutton Church (both in Kent) also fell down. At these places, and also in East Kent, three shocks were felt—at 6 p. m., 9 p. m., and 11 p. m. The shock of 6 p. m. was also felt in France and Belgium.

1816, August 13. This is the strongest known earthquake felt, not only in the Inverness district, but in all Scotland. In Inverness the injury to property was considerable. Chimney-tops were thrown down or damaged in every quarter of the town; many slates and tiles fell from roofs; and several walls were cracked. In the Mason Lodge ——— one of the coping stones (of the chimney) estimated to weigh 50 or 60 pounds, fell on the other side of the street, a distance of not less than 20 yards. The octagonal spire of the county jail was broken through about 5 or 6 feet from the top, and the upper part was twisted round so that the angles of the octagon were turned nearly to the middle of the flat sides below.

After describing the shocks which occurred in Wales between 1690 and 1841 the writer says:

Only one of these early shocks [1832] was comparable in strength with the remarkable earthquakes of Pembroke in 1892, Carmarthen in 1893, and Swansea in 1906. All three were twin earthquakes of the second class. They belong to the front rank of British earthquakes—their intensities were 7, 7, and 8 respectively [on the Davison, modified Rossi-Forel scale, on which 8 is apparently about equivalent to the VIII of that scale as usually interpreted] while the areas included within the isoseismal 4 were 44,860, 35,900 and 37,800 square miles.

In weighing these accounts one should bear in mind the character of British architecture; while of great strength, the structures are excessively heavy and their massiveness renders them peculiarly liable to damage by their own weight. The inference is justified that Great Britain has not experienced a shock of major violence since some historically very re-

mote, though perhaps geologically not distant epoch, if ever.

The moderate intensity of British earthquakes may, perhaps, afford a reason for the special scale of intensities employed by the author. He describes it as a modified Rossi-Forel scale. It is not apparent what has been gained by the modification, which seems to consist in the adoption of one test only for each degree of the Davison scale; thus: "5. The observer's seat perceptibly raised or moved. 6. Chandeliers, pictures, etc., made to swing. 7. Ornaments, vases, etc., overthrown. 8. Chimneys thrown down and cracks made in the walls of some, but not many, houses in one place." 9. The same, but "cracks made in the walls of about one half the houses." It would seem that a shock which would perceptibly move an observer's seat, even John Bull's, would cause a chandelier to swing and might well overthrow a more or less delicately balanced vase. Or considering the criterion for intensity 5; how could it be applied during those hours when people generally were in bed? Yet the author lists as of intensity 5 a number of shocks which occurred during the wee, small hours. It is regrettable that in seeking precision he should have added to the already grave confusion.

With reference to the mechanics of earthquakes the author accepts the generally recognized theory of elastic rebound as the explanation of all British shocks. The theory was clearly established by the California earthquake of April, 1906, where obvious displacements of blocks of the earth's crust were observed. Similar displacements have not been recognized in Britain. The faults which have been identified there are ancient, even geologically, and the inactivity of the Atlantic basin as contrasted with the activity of the Pacific marks a contrast in degree if not in kind. It is not impossible that there are other mechanisms in the earth's crust besides faults which may give rise to elastic waves and it would be wise not to push assumptions too far. Where faults are known to occur in the vicinity of earthquake foci, as is the case with the Great Glen and Highland Border faults, the evidence favors the theory; elsewhere the reader may be pardoned if he retains an open-minded doubt.

The work contains a few closing pages in which the author describes the phenomena of twin earthquakes and seeks to explain their origin: discusses the conditions of pressure that produce folds in the earth's crust; and analyzes the relations of folds to earthquake faults. Shearing, the mechanical effect which was first recognized by Reid as the condition of development of the California faults when he applied the rebound theory, is not considered by the author as a cause of British faults. Yet shearing is the one mechanical effect which can produce planes of displacement at right angles to each other, that is, in the relative positions in which the trends of twin earthquakes lie with reference to each other, according to the author, in England. It is in fact impossible that folds should develop at the depths which are assigned to the foci of twin earthquakes, since at those depths the character of the rocks and the loads they bear combine to make bending more difficult than shearing. It is also true according to the principles of mechanics that two forces acting simultaneously upon a given body must produce a single resultant. One can not produce an effect independently of the other. This elementary principle appears to have been overlooked by the author who refers to "compression in two directions" and to folds which constitute a "double system of corrugations." To quote: "If we may assume that the earthquake faults are approximately at right angles to the folds to the growth of which the earthquakes are due, it would seem that the crust at a depth of a few miles below the counties of Stafford, Derby, and Leicester is corrugated in two systems of perpendicular folds." It is in assuming folds that the seismologist has gone astray. Had he assumed shearing, due to compression along a diagonal of the nearly rectangular system of the two series of faults, the explanation would have been in accord with mechanics as well as with observation. His mistake is the more natural since many geologists write of folding the earth's crust without the least consideration of the mechanics of the process and its limitations, but it vitiates the theoretical concept of a structure in which folds are assumed to play the dominant rôle.

As an authentic history of British earthquakes, which assembles the data in a most carefully considered statement of facts and covers a thousand years, Professor Davison's work is a most timely contribution to the study of seismology on both sides of the Atlantic. The geologic histories of Great Britain and the eastern United States have run parallel with such nicety during so many ages that the subterranean forces, which have acted and are acting to disturb the landmasses, may well be assigned to a common dynamic source, namely, energy set free in the rocks beneath the Atlantic basin. The earthquake record of the New England-St. Lawrence province during the last three hundred years has resembled that of Great Britain, except that the shocks which have been experienced appear to have affected far larger areas and may have been somewhat more violent. Even so, Great Britain's record of a thousand or more years without an earthquake of the first magnitude is reassuring for her sons across the water.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

METHODS FOR THE DETERMINATION OF REPLACEABLE BASES IN SOILS

IT is not the purpose of this preliminary paper to discuss the general subject of base replacement in soils or to suggest its importance in soil economy. These phases of the subject have already received extensive attention by Van Bemmelen, Gedroiz, Hissink, Kelley and others. Our object is rather to advance a method whereby such replaceable bases may be readily determined quantitatively and with a minimum expenditure of time and effort. Writers on this subject from its inception record attempts to perfect such methods, but even the most recent contributions deplore the fact that to-day no procedure is at hand which gives accurate results alike with either acid or alkaline soils and with soils carrying organic matter in any considerable amounts.

The displacing solutions most commonly employed have been those of NH_4Cl, KCl and NaCl, but, as it is often desirable to determine replaced K and Na, the latter two salts are not always applicable. We thus have NH_4Cl left as the salt almost universally used for this purpose. It is unsatisfactory for three major reasons. First, where sufficiently large samples of soils are extracted to give proper amounts of Ca, Mg, Na and K for accurate gravimetric determination, and where tenth-normal (or stronger) solutions of NH_4Cl are employed, there remains upon evaporation an enormous amount of NH_4Cl which must be gotten rid of before these determinations can be made. This is often accomplished by volatilization, and, as is well known, where the amounts of NH_4Cl are large (many grams) and the amounts of Ca, Mg, Na and K small (often but a few milligrams) the possibilities of error due to mechanical loss are great. Furthermore, such a procedure is time-consuming and requires constant attention. Repeated evaporization with HNO_3, as used in some laboratories for this purpose, is also wasteful of time and disagreeable. The second, and, to us in the arid southwest, a very important reason for condemning NH_4Cl is that $CaCO_3$ is appreciably soluble in its solutions (280 ppm in tenth-normal NH_4Cl). The soils of this section are, almost without exception, calcareous, varying from 1 to over 50 per cent. $CaCO_3$; hence any solution which appreciably dissolves this salt is of questionable value where replaceable Ca is to be determined. Magnesium carbonate (finely ground magnesite) is also soluble in solutions of NH_4Cl, but to a lesser degree. The third reason for censuring NH_4Cl as a replacing agent is that it dissolves and removes from the soil large amounts of organic matter (especially from alkaline soils) which, as a soil colloid, may have a bearing on the replacement reaction.

The method which we propose for the determination of replaceable Ca, Mg, Na and K in mineral soils follows. A tenth-normal solution of $BaCl_2$, as suggested by C. S. Scofield, was finally adopted as the replacing agent, and for the following reasons: $CaCO_3$ or $MgCO_3$ are less soluble in it than in 10th-normal NH_4Cl; it does not appreciably dissolve soil organic matter; it is exceedingly active as a replacing agent. Five hundred grams of air-dry soil are placed in a glass percolation tube and leached with tenth-normal $BaCl_2$ solution until free from replaceable Ca (1,000 to 1,500 cc). The percolate is then made up to a definite volume with distilled water, and if at all turbid (which is not usual) filtered through a porcelain pressure filter. Two hundred cc are placed in a 250 cc graduated flask, 4 or 5 drops of con. acetic acid and 20 cc of sodium chromate solution (175 g. $Na_2CrO_4\,10H_2O$ per liter) are added in the cold to precipitate the Ba. The whole is made up to 250 cc, thoroughly agitated, and allowed to stand over night to clear. Careful examination has shown no absorption of either Ca or Mg by the $BaCrO_4$ precipitate. Two methods of procedure are here possible. The Ca and Mg may be determined by the soap titration method, or by the standard methods, after separating them from the chromate solution. Both are often used in this laboratory as checks. A full description of the soap method as modified by us will be given in the detailed report of this work soon to appear. Where the standard methods are to be followed, 150 cc of the clear, supernatant liquid are pipetted into a beaker and the Ca and Mg precipitated completely as insoluble phosphates by adding an excess of 2 per cent. ammonium acid phosphate solution in the cold and making alkaline with NH_4OH, as in the ordinary precipitation of Mg. Let stand over night, filter, wash with 2 per cent. NH_4OH, dissolve the precipitate in a small amount of hot dil. HCl, make up to a volume of about 100 cc with distilled water and bring to a boil. Add *just enough* NH_4OH to make *slightly* alkaline and throw out the phosphates, then with stirring add 10 cc of a 5 per cent. solution of oxalic acid and boil. The solution should now be distinctly acid, while the Ca will be precipitated completely as the oxalate. Filter and determine the Ca by titrating with a standard solution of $KMnO_4$. Evaporate the filtrate to a bulk of about 50 cc, cool, add 2 or 3 cc of the ammonium acid phosphate solution and precipitate the Mg as $MgNH_4PO_4$ by making alkaline with NH_4OH in the usual way, finally weighing as $Mg_2P_2O_7$.

Sodium and potassium are determined in the original $BaCl_2$ percolate as follows: an aliquot (100 cc or more) is placed in a porcelain evaporating dish, slightly acidified with HCl and 5 cc or more of a 14

per cent. solution of $(NH_4)_2SO_4$ added. Evaporate to dryness on the water bath, dry in the oven and drive off slight excess of ammonium salts in the electric muffle at low heat, finally heating to dull redness. Take up in dil. HCl, digest on the water bath for an hour or more, filter and determine Na and K by Hilgard's methods. The $BaSO_4$, *after heating,* has little or no tendency to absorb Na and K. This, however, is not true in the cases of Ca and Mg.

Where the soils carry soluble salts ("white alkali" soils),[1] the soil in the percolator must first be leached free from these salts with pure water before being subjected to the $BaCl_2$ treatment, or, where percolation with water is slow as is the case with heavy soils, a separate analysis may be made for water soluble bases and these subsequently subtracted from those found in the $BaCl_2$ solutions.

In the case of acid soils (unsaturated with respect to bases), the amounts of replaceable H-ion may be determined in the $BaCl_2$ percolate by means of titrations as recommended by Gedroiz and recently modified by Joffe and McLean.[2]

Representative data, solubility determinations and certain other work leading up to the adoption of these methods is in press in the technical series of bulletins of the Arizona Agricultural Experiment Station.

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SPECIAL ARTICLES

ANALYSIS OF THE COLOR OF THE SKIN AND ITS SIGNIFICANCE

OUR complexions and the colors of the skin are dependent on a variety of causes. Recent experimentation by methods to be briefly described here indicates that these causes include (1) pigmentation, or melanin and (2) the character, amount, distribution and velocity of flow of the blood in the capillary bed in the skin. Normal blondes and brunettes differ in the amount of pigmentation only. It is said: "That the pigmentation of the skin bears some relation to health is well recognized. Tanned individuals are generally healthy. But whether the action is a direct or indirect one is unknown."[1] I have found, however, that the condition of tan per se, due to pigmentation, is not such an indicator because of the fact that the blood, by reason of the reflection of light from the capillaries in the surface of the skin, contributes its quota to the composite color of the skin as seen by the eye. The eye is a poor instrument for analyzing or resolving the constituents of color.

Tintometer methods for estimating and recording the color of the skin may be of value to the medical profession in the course of the treatment of diseases in which appreciable color changes occur in the skin, but they do not in any manner analyze the light reflected by the skin and therefore can not record skin color in terms of the three attributes of color, that is, brilliance, hue and saturation. Spectrophotometric analyses of the light reflected by the skin and the subsequent analysis of these data into the components of red, green and violet excitation values and relative luminosity on the basis of the noonday sun as a standard furnish the only scientific method for obtaining information on the rôle played by pigment and blood in the color of the skin. So far as I know, no work has previously been done on this subject. This paper is a preliminary report of my recent investigations.

Accurate determinations of the color of the skin in terms of spectral wavelengths and amounts, with the subsequent analysis of such data, are doubtless of decided value in cases of jaundice, cyanosis, polycythemia vera (in which capillary dilatation and increased cell volume play a part), Addison's disease, hemochromatosis and anemia.

The instrument employed in these investigations, which will be reported in detail elsewhere in medical literature, is the Keuffel and Esser color analyzer. Reflection curves from various portions of the fingers or hands may be obtained in terms of the reflection of light from a block of magnesium carbonate by placing the hand or fingers in the position ordinarily occupied by the second magnesium block. I have also built and adapted to this spectrophotometer an attachment whereby the face or arm may be used and reflection curves obtained.

Figure 1, curve 1, shows the spectral distribution of light reflected from the fingers of a normal blond; curve 2 gives similar data in a case of Addison's disease; curves 3 and 4 are from two cases of polycythemia vera. In each instance corresponding fingers and areas were used. The ordinates give the percentages of reflection, while the abscissae give the wavelengths as read on the drum of the spectrometer.

These data as plotted in Figure 1 (and other similar data) have been analyzed into percentages of red, green and violet color excitation values and relative luminosity with noonday sunlight as the standard, in the manner described in detail in the report of the

[1] Soils carrying "black alkali" must be treated differently. A discussion of this case will appear in the bulletin referred to later.

[2] "Colloidal Behavior of Soils and Soil Fertility: II. The Soil Complex Capable of Base Exchange and Soil Acidity." In *Soil Science,* vol. 21, no. 3, pp. 181–195 (1926).

[1] Matthews, "Physiological Chemistry," 4th ed., p. 711, 1925.

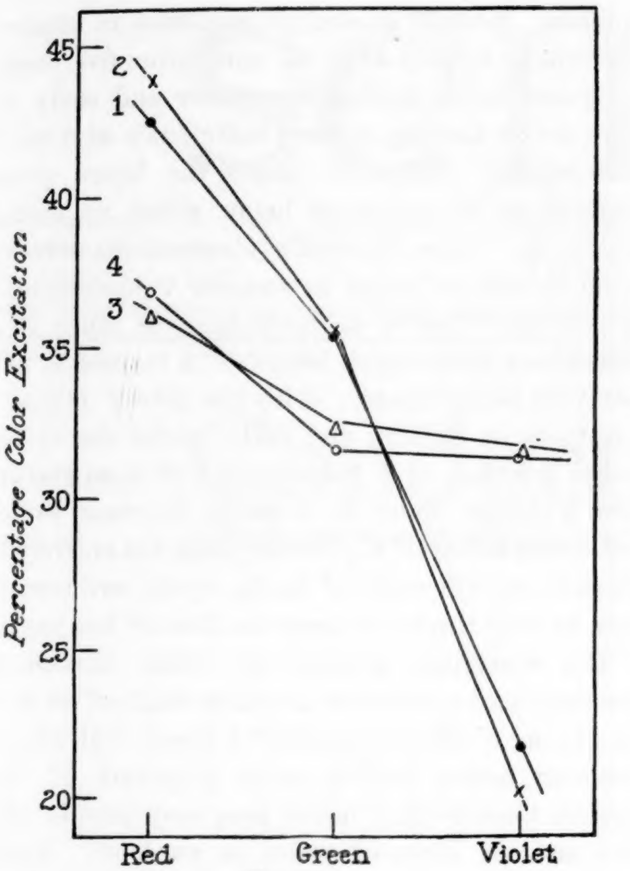

FIG. 1

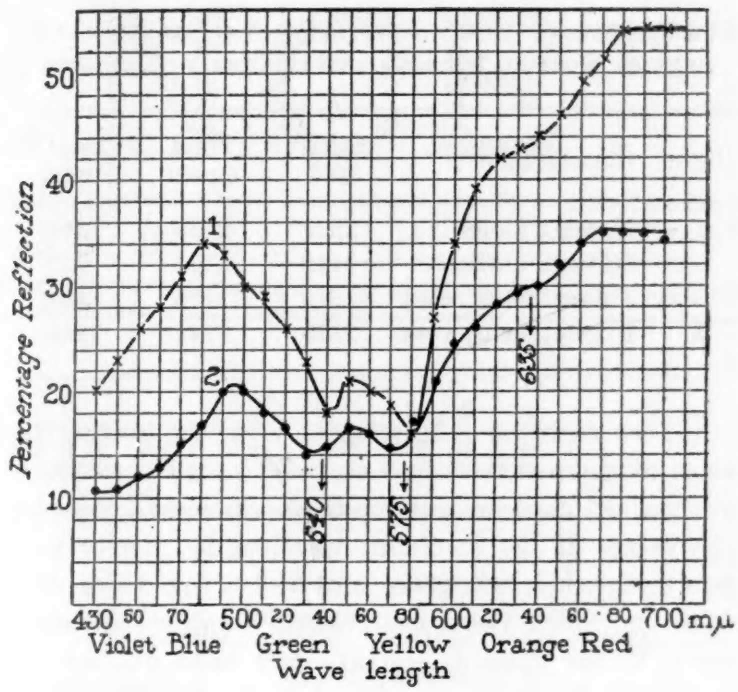

FIG. 2

Committee on Colorimetry of the Optical Society of America.[2] Figure 2 gives the percentages of the red, green and violet excitation values in the reflection of light from the skin in a case of (1) normal blond, (2) Addison's disease, (3) polycythemia vera and (4) marked polycythemia vera. Table 1 also gives the dominant wavelengths and purity in each of these cases. Table 2 contains the summated color excitation values of red, green and violet in each of the four conditions cited.

The following conclusions are drawn from the experimental work conducted thus far:

(1) The spectrophotometric method of analysis of skin color furnishes the basis for a relatively simple but bloodless method of depicting certain characteristics of the blood at the peripheral portions of the body.

[2] Troland, "Report of Committee on Colorimetry," *Journal of Optical Society of America*, Vol. vi, pp. 527–596, 1922.

(2) The spectrophotometric reflection curves show the presence (or absence in certain cases) of (a) hematin, (b) reduced hemoglobin, (c) oxyhemoglobin and possibly (d) methemoglobin in peripheral or surface tissues and blood.

(3) It is probable that the spectrophotometric data as obtained from the skin by reflection methods may be of value in determining certain physico-chemical characteristics of the blood.

(4) By spectrophotometric reflection curves and their subsequent analysis into fundamental red, green and violet excitation values, it is possible to differentiate between changes or variations in skin color due to (a) pigment content and (b) characteristics dependent on the amount, distribution or quality of the peripheral blood supply.

(5) Spectral analyses show that there is very close agreement in the values of the dominant wavelength, purity and percentages of red, green and violet in normal blondes and brunettes and in such pathologic conditions as Addison's disease. The dominant wavelength is practically 585 μ (nearly sodium yellow) in all instances.

TABLE 1

PERCENTAGE EXCITATION VALUES DETERMINED FROM THE SPECTROPHOTOMETRIC DATA ON THE SKIN OF NORMAL PERSONS AND IN CERTAIN DISEASES

Case	Curve (Fig. 1)	Classification	Red	Per cent. Green	Per cent. Violet	Dominant wavelength (μ)	Purity, per cent.
1	1	Normal blond	42.45	35.68	21.87	587	43
2	2	Addison's disease	43.90	35.70	20.40	580	50
3	3	Polycythemia vera	35.81	32.49	31.70	605	10
4	4	Polycythemia vera	36.90	31.60	31.50	640	10

TABLE 2

Summations of Color Excitation Values from the Skin of Normal Persons and in Certain Diseases

Case	Classification	Total Red	Total Green	Total Violet
1	Normal blond	2600	2186	1340
2	Addison's disease	1412	1151	650
3	Polycythemia vera	1570	1425	1388
4	Polycythemia vera	1330	1141	1130

(6) In cases of polycythemia vera, the congestive stage of Raynaud's disease and allied conditions there are marked departures from the normal conditions in the values of the dominant wavelengths, purity and percentages of red, green and violet. In these diseases, in which there are generally marked disturbances in the quantity, quality and rates of flow of blood, the dominant wavelength is in the red (605 μ to 640 μ in my findings).

(7) Cyanosis is noticeable in various diseases. Spectrophotometric determinations and analyses in terms of monochromatic radiation show that the summated violet values are higher by percentage in comparison to similar summated values of the violet in normal subjects, and that there is a marked reduction in the summated values of the reds and greens in these diseases as compared with normal persons.

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CILIARY ACTIVITY OF THE OYSTER

In a recent paper[1] Galtsoff describes an ingenious method for determining the rate and the amount of water passed through an oyster's gills. Since these experiments were conducted in a tank in the laboratory it is of interest to compare Galtsoff's findings with observations of oysters in their natural surroundings.

As was to be expected, following the work of Gray ('23)[2] on the effect of temperature upon ciliary movement in the gills of *Mytilus,* the rate of flow of water through the oyster was found to be a function of the temperature. Unlike Mytilus, however, the cilia of the oyster were seen to come to a standstill at 5° C. with no current produced below 7.6° C.

From examination of the stomach of oysters throughout the year I showed[3] that between 5.6° C. and 7.2° C. lies a "critical temperature" above which active feeding occurs and below which almost no food is taken. Round[4] showed no reduction in numbers of bacteria in oysters at 5° C. until after five days.

Oysters taken during the winter and early spring show active feeding in some individuals at a temperature of 5.6° C., or 2° below the figure given by Galtsoff as the minimum below which no current is produced. Since Galtsoff's observations were made upon oysters adjusted to summer temperatures it is evident that *Ostrea elongata* behaves much like the ctenophore *Mnemiopsis leidyi*[5] with respect to its temperature adjustments. With the slowly falling temperatures of autumn and early winter the oyster becomes adapted to a lower range of temperature, so that although there is a sharp decrease in ciliary movement below 5° C., activity does not entirely cease. Theories of hibernation[6] in the oyster are true therefore in only a relative sense as Round[4] has suggested.

The maximum amount of water filtered by a medium sized oyster was found by Galtsoff to be 3,000 cc. Using a different method[7] I found that when commencing active feeding after a period of closure, oysters three to four inches long may take in between five and six liters of water in an hour. Extended observations of oysters attached to a kymograph while living under natural conditions show that the rate of feeding in the oyster may be subject to wide variations independent of changes in temperature, turbidity, salinity and other environmental conditions. Feeding occurs, for example, much more actively on the flood than during the ebb tide.

Galtsoff found that his oysters living in the laboratory under controlled conditions kept their shells open an average of twenty hours each day. With oysters living under natural conditions, exposed to wide variations in temperature, turbidity, salinity and pH I found[7, 8] that out of each day the oyster remained closed for an average of four hours, most of this period of inactivity occurring at night. The agreement between Galtsoff's findings on oysters in the laboratory with mine on animals living under natural conditions indicates that the period of closure in the oyster represents a minimum of physiological inactivity determined by the needs of the organism. To quote an earlier statement of mine on this subject (7, p. 340), "the writer believes the evidence from all sources indicates that the periods of inactivity which occur under conditions favorable for feeding are to be looked upon as true rest periods."

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1 Science, 63, No. 1626.

2 Proc. Royal Soc., Ser. B., 95, No. 664.

3 Rep. N. J. Expt. Sta., 1921, p. 293.

4 Rep. R. I. Com. Shellfisheries for 1914.

5 *Biol. Bull.,* 48, 92–111.

6 Gorham, F. P., Rep. R. I. Com. Shellfisheries for 1910, and *J. Am. Publ. Health,* January, 1912.

7 Rep. N. J. Expt. Sta. for 1920.

8 Proc. Soc. Exp. Biol. and Med., 21, 91.